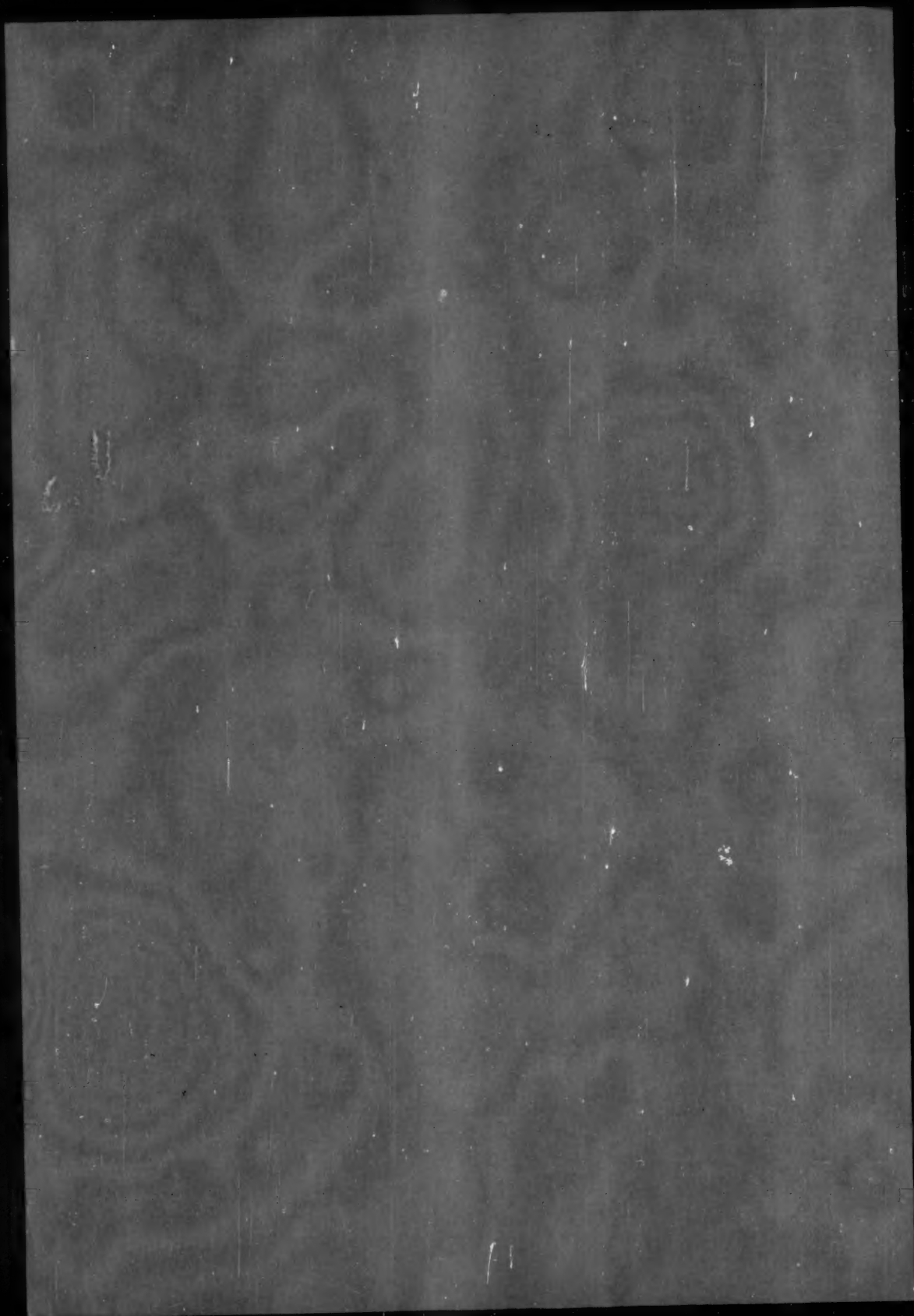


# **THE AMERICAN JOURNAL *of* PSYCHIATRY**

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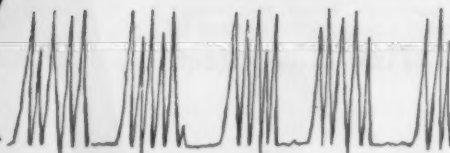
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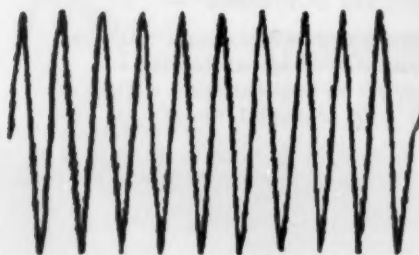


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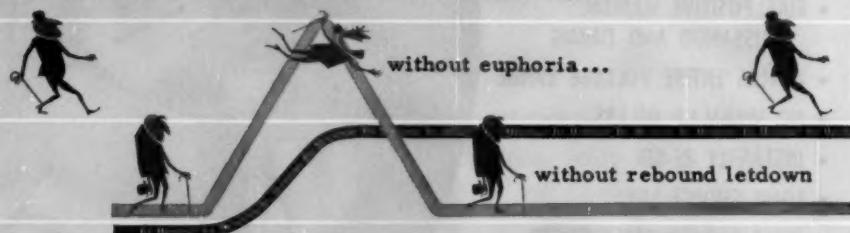
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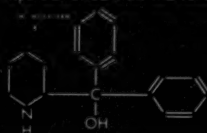
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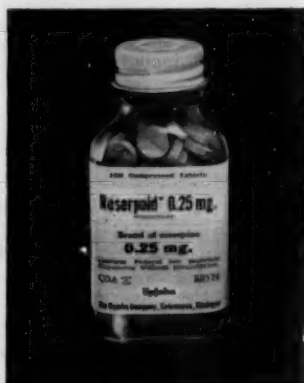
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ON DISCOVERY AND EXPERIMENT IN PSYCHIATRY<sup>1</sup>HENRY W. BROSN, M.D.,<sup>2</sup> PITTSBURGH, PA.

Statesmen and scholars alike are publicly asserting the cash value of new ideas in all fields of science. Probably because of the pressures of war, both past and present, there is an increasing acceptance of the advantages to commerce, industry, and the military establishment of a large research program. In fact, we are told that not only are new developments essential to maintaining our standard of living and our Western culture, but they are absolutely necessary for survival (36, 37).

Medical students and psychiatric residents are furnished many facts regarding the nature of mental illness and the ways in which it may be prevented and treated, but even though these basic facts and the methods by which they were obtained are available, they do not become for most students a clear-cut living imagery with which they are comfortable, or which impels them to intensive further study at the frontiers of investigation. This is as true of the fields of organic neurology, clinical neurophysiology and neurobiochemistry as it is of experimental psychology and psychodynamics. I do not know the reasons why most residents show relatively little interest in doing some independent research along with their therapeutic work, or why a few more of them are not impelled to research as a career. Poor economic inducements and lack of emphasis upon independent participation in project work in the curriculum are sometimes cited, but I shall later take up some of the psychological factors which seem relevant to me.

However, since we are already committed to placing more resources in these fields, it is vital that physicians and psychiatrists inform themselves of the central issues. They may then take their appropriate roles both as influential citizens and as active contribu-

tors to the vast developments which are now in their infancy. Physicians can exert untold influence upon laymen about the research of the future if they are well informed and have conviction about the best methods to receive support from universities, foundations, and the Federal agencies. They have a duty to do so, since they have much valuable personal experience to bring to this field, and it would be wasteful to disregard this only to be forced to return to this neglected topic at a later date. The conscientious physician will want to know more about the intimate nature of the arts of invention, discovery, and experiment so that he may support those men who seem to have the required abilities to make significant contributions. He will then also see the reason why most research is gambling at a high level where tangible results are not easy to assess, that many important contributions are by-products of experiments designed for other purposes, and that most research is a grinding routine with phases of careful evaluation of evidence and alert criticism of oneself and others. The responsible physician will see the need for research about research, a sympathetic but close examination of the men who do the work, and the conditions under which they labor.

I hope it will appear from later material that this task is not an impossible one provided that the goals are extremely modest and the methods to be employed remain under careful public scrutiny. Admittedly, we have a relatively limited insight at this time into the dynamics of productivity, but the improved methods of dealing with subjective and introspective material encourage more study. If we continue to identify the sources of erroneous judgements in the observer we find many problems in psychodynamics less puzzling. Freud said, "Let us see the unclear, clearly." The terrors of self-knowledge will diminish as we become more familiar with these dynamics and welcome the support and satisfaction which come from them. While the artist and scientist often work at levels of greater intellec-

<sup>1</sup> Read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954.

<sup>2</sup> Director, Western Psychiatric Institute and Clinics, Professor and Chairman of the Department of Psychiatry, University of Pittsburgh School of Medicine.



tual complexity they also need warm human appreciation in their arduous tasks. If we can help the men who are engaged in research in a more skillful manner because we know something about it as a human enterprise with pitfalls and limitations, we shall not be so driven to find projects dependent upon mechanical methods which do not promise significantly new rewards. With such understanding, we shall be more willing to place our assets at the disposal of gifted men with a greater confidence that they will be able to carry on more successfully than their predecessors.

So far we have stressed the advantages which accrue from research about research methods and, more importantly, research about the men who do the work and the best conditions for productivity. Perhaps the story Dr. Alan Gregg tells about the Oxford scholar will help us set the stage for learning something about desirable future methods of teaching and learning. The Oxford scholar (probably Sherrington) said that they knew fairly well how to teach that which was known, but it was now necessary to teach ways of meeting the unknown. While some may protest that this has always been so, it seems to many others of us that the rapid accretion of new knowledge makes older methods of assimilation inadequate. The inertia of some learned men to new ideas on occasion is all too prominent in the history of science. Freud pointed this out in his 1909 supplement to the first chapter of *The Interpretation of Dreams* (21).

... it has, of course, received the least attention from the so-called "research workers on dreams," who have thus afforded a brilliant example of the aversion to learning anything new so characteristic of the scientist. "*Les savants ne sont pas curieux*," said the scoffer, Anatole France [p. 186].

We find upon examination that the qualities of courage and enduring persistence, more than imagination and technical skills, important as these are, distinguish the majority of notable contributors. To some, the problem of how to encourage the vital qualities in our young students and then how to select those with the most promise would seem one avenue to providing highly needed manpower in the research field. Since most children have imagination and curiosity until it is lost

in the process of growing up, we might ask the specialists in child development, child psychiatry, and primary education what contributions they can make to this problem. Time does not permit us to examine the possibilities here, but we can venture the guess that there are unheard of potentialities in helping children to grow up with desired abilities and still not create little monsters of intellect, if we worked on the possibilities now known to us. Some educators favor a strong secondary school education which at once furnishes a substantial training in the rigors of acquiring sound information, but also provides through elective courses and independent projects the opportunity for maximal independent growth. Some preparatory schools and colleges have worked hard on this avenue and, at this time, we should be able to see some of the results. Perhaps it will require another generation to know which methods best seem to furnish us with dedicated investigators. In a goodly number of such men there seems to be no doubt that they received their strong stimulus from a significant figure, a teacher who had the ability to fan the desire of the student to learn much about a subject and also to convince him that a highly satisfactory way of life was to be found in the university or in a laboratory. The influence of Isaac Barrow upon Isaac Newton is an intriguing example (1). The theme of the significant figure recurs frequently in reviewing biographies of prominent men although sometimes he was not found until college or more rarely in postgraduate work. This teacher need not necessarily be a well-known man or even an industrious publisher, but he did have the time and interest to spend with the inquiring pupil so that the latter could reorganize his life goals, working habits, and his self-image into a sufficiently stable structure to carry him into adult life. Correlative features such as race, religion, economic and cultural background seem to be less important in a large study reported on this subject, although one may find some suggestive hints that these are important (8, 29, 30, 31, 45, 46, 47, 48, 55).

Some educators would stress more science with more laboratory work in college, but this has not apparently been very successful



with either engineers or medical students. It is suggested that to give the future physician a better knowledge of a living man in his social environment, he should have thorough elementary courses in experimental and social psychology, social anthropology and sociology, or similar disciplines. For the more interested student, training in statistics and sociometrics could be provided. In addition, the imaginative humanist could make a good case for the young pupil to study the classics for a searching introduction into psychodynamics, since many novels, plays, and biographies furnish a splendid proving ground for such exercises at the intellectual level and without grim clinical responsibility. While one may applaud these proposed curriculums, it is not probable that the medical students of the near future can come to medical school as sub-specialists in several fields, as well as having a good general education(16, 17). Nor can we expect the revised medical school curriculums, now in experimental stages, to assimilate the functions of these preparatory courses. No doubt adequate compromises will be worked out in the future when medical faculties can decide more definitively what they want to teach most and in what order, so that the preparatory schools and colleges can assist more intelligently in the total process.

There has long been a tradition in some schools that more free time in the medical curriculum, for both elective work in clinics and laboratories, would provide the medium for giving students an opportunity to identify with competent investigators, to learn the methods of science and the style of life which would encourage those best fitted to stay in research work(9, 33). In those schools with which I am most familiar, some residents are put to work (1) on problems of their own choosing, (2) on ongoing projects under direction of senior staff men, or (3) on joint work with other men under supervision. Some clinics try to get students or residents started as soon as possible on projects in addition to their regular clinical work. Because many residents are not able to respond enthusiastically to the added burden, other clinics adopt the method of "farming out" interested men to act as assistants for a 6-months' period to a laboratory or an

ongoing research program. This method meets with most approval but much closer examination is needed into the dynamics of the interactions and also the results(9).

It is a real question whether a man during this warlike decade can become deeply interested in investigative work as a serious career or as a significant part of his daily work as a clinician, after the age of 25, if he has not had strong early indoctrination, and after he already has made profound commitments elsewhere. There has always been a scarcity of genuine talent in our field, and in spite of occasional exceptions, most residents have not had enough identification with the rewards of research to withstand the attractions and satisfactions of clinical work. In clinical work itself relatively few feel the drive to do independent investigation. Some who are impelled to try find it insufficiently rewarding, while others do it so badly that they find good reasons to stop. Time does not permit a detailed discussion of the different qualities which characterize a clinician or academician as differentiated from an experimenter. There is no doubt that many men can do important experimental work in clinical psychiatry just as in clinical medicine, but the problem becomes more difficult as the experiments become more psychological in a manner which does not permit relatively easy recording, comparisons, repeated trials, and public verification. The clinician working with a profound attachment to the single case is not in as flexible a position to manipulate the material presented to him by a patient, as the experimenter who has as his primary obligation and interest the problems rather than the total person. This does not mean that material from single cases is not useful or rewarding. Quite the contrary, these may be the best sources of new ideas which will markedly alter our theory and practice. Nor does it mean that psychiatrists should not use currently acceptable methods of statistical, sociometric, physiologic, and biochemical methods to assist them whenever possible(20, 24, 26, 39, 43).

Curiously, there are a goodly number of young men who have had excellent scientific training in laboratories with outstanding teachers for several years and yet do not find investigation either essential to them-

selves or a major source of satisfaction. All of us are acquainted with young men of promise who contribute relatively little after an auspicious beginning. Another contingency, of no small importance, is obvious waste entailed in training a man for many years in a research discipline only to have him lose his interest relatively early in middle life. The half-life of a scientist is often regrettably short. One unrewarding pattern is that of the man who is driven by various inner needs and social pressures into a career of investigation only to find that the same motivations which sparked his interest prevent his being a genuinely productive worker. It might be said that these somewhat restricted men are a necessary cost to the new industry of research and that they also serve other useful functions, even if they are relatively inefficient in frontier research. They point up the fact that this is but one of many problems which need careful long-term study in the crucial conflicts which perplex the investigator. Kubie(32), from his nearly 30 years' experience, writes a vital and fascinating paper on the emotional equipment of a young scientist, the choice of a career, the neurotic distortions of scientific research, and the influence of unconscious symbolic processes on the production of logical thought and logical error in scientific research, which I can recommend highly for a common-sense approach to the questions just presented. The reprint of these two sections contains valuable additional clinical material (pages 21-23), which was omitted from the original articles in order to economize space. I regret that space does not permit citations of his case material. After a great deal of verified information of the type Dr. Kubie describes in his first article is available, we shall better be able to study the world of the investigator.

Three monographs by Anne Roe(46, 47, 48) describe investigations about

... the existence of relationships between life history, intellectual functions or personality characteristics, and the selection and pursuit of a particular science as a profession. This has been the first series of its kind in this field, and hence the major approach has had to be observational and diffuse. In so complex a problem, the first need is to get some idea of the nature of the relationships, if any exist, the points at which a direct attack can be made, and the sort of tools to use. It was felt that

no existent personality theory was sufficiently developed, or generally suitable for the derivation of hypotheses in advance. Now that extensive observations have been made in this specific field, for this specific purpose, it is possible to set up a number of hypotheses concerning these relationships which can be checked directly in future work (48, p. 1).

The outstanding achievement of Dr. Roe in obtaining this unique material from prominent men active in research, and her careful interpretations deserve our closest attention. One might wish that she had permitted herself more freedom in interpretation of motivations while she had this unusual opportunity, but one can also respect her discretion. Her willingness to use the simplest empirical method of observation, recording, and comparing, even at relatively simple levels, also deserves mention, because this willingness, as we shall discuss later, may be one of the first steps to a more meaningful research.

Kubie and Roe both present socio-economic data regarding the choice of profession and related topics. They paint a realistic but grim picture of the self-sacrifice required from the man, but even more so, from his family and that with a most uncertain reward in terms of fame. One well-known authority, Dr. Alan Gregg(25), who has had a most intimate acquaintance with scientists for the past generation, tells much the same story. Other writers on medical research on an impersonal level can supply some additional social perspectives about current practices(13, 38, 52, 53).

These observations will call attention to the need for long-term planning over many decades in order to find and train talented men to fill the need. The suggestions that follow for the teaching of medical students and residents in psychiatry are not intended as pressures to steer men into research or to train experimenters. They are only suggestions to teachers about ways in which the physician can understand science, not merely facts, and the men who work at it, somewhat better in order to aid them individually as persons and also to provide scientists and laymen with informed opinion and support. Many experienced and talented investigators believe that skills in experimental methods can be acquired only by active par-

ticipation in a productive laboratory. Perhaps we teach too much to permit a man to cultivate his own growing powers. Some are even more pessimistic and believe that really good experimentalists are born, not made, and that these natively endowed men can learn relatively little from either didactic courses or colleagues. What may be needed is the ability to furnish able men with the opportunity to make creative errors without imperiling them. Sooner or later some of the attempts will be significant. This is well illustrated in Ernest Jones's biography of Freud (28).

What can we do now to help our medical students and residents gain a better idea of the circumstances surrounding the appearance of new ideas? We have already mentioned various plans for participation and instruction. Obviously elementary didactic courses, however comprehensive, do not meet the real problem. Actual research work in or out of the clinic or laboratory is not always feasible. Any number of pedagogic devices have been proposed and we shall discuss a few of them briefly. Perhaps one of the best would be a seminar course in which investigators would come in to tell their own story about the origins, growth and development, and trials and tribulations of their own work. It is a real question in my mind if they should be asked to do much teaching of this kind, unless they themselves find it rewarding. Usually they have enough obligations without trying to examine their own relations to their methods and materials in public. Few of them could or would have the ability to detail their own tribulations to students *en masse*. Such confidences must be earned by long association and a deep-rooted respect. The value of the case histories of Kubie and Roe is that they do transcend the conventional limits.

The case history method has proven itself of value, not only in the widely known sense of Osler in medicine, but in law schools and in the outstanding work of Elton Mayo and his pupils (35) in the field of industrial relations. J. B. Conant, in the 1946 Terry Lectures (15), develops the thesis that the case history method of studying both discoveries and experiments is much superior to the usual didactic methods of teaching physics

because it enables the student to gain more intimate understanding of attitudes and feelings toward science as opposed to mere acquisition of theories or experimental results. In spite of the voluminous output of his critics, he does not favor the study of the formal aspects of scientific method for laymen or young students (15).

... for the histories illustrating the Tactics and Strategy of Science are as yet unwritten. But in spite of this lack of competition I doubt if the philosophical treatments of science and scientific method have been very successful when viewed as an educational enterprise. No one questions of course the importance of this type of penetrating analysis. There must be constant critical appraisal of the progress of science and in particular of scientific concepts and operation [p. 13].

He stresses the fact that textbooks are devoid of descriptions of the hurly-burly and confusion which surround the scientist and may not infrequently be present in himself (15).

Now, of course, there is no question that one of the necessary conditions for scientific investigation is an exact and impartial analysis of the facts. But this attitude was neither invented by those who first concerned themselves with scientific inquiries, nor was its overriding importance at once recognized. As one skims the histories of the natural sciences, it seems clear that in the embryonic stages of each of the modern disciplines, violent polemics rather than reasoned opinions often flowed most easily from the pen [p. 6]. . . . Would it be too much to say that in the natural sciences today the given social environment has made it very easy for even an emotionally unstable person to be exact and impartial in his laboratory? [p. 7]

These frank statements about the high degree of subjectivity and inner conflict in most active investigators can be substantiated from many other noteworthy men to be cited later. Note that we in the psychological and social sciences do not get such safeguards as tradition, accurate instruments, high degree of specialization, or witnesses against distortion of evidence and loose interpretation which were built up in the physical sciences during the past years. Yet most of us in reading about science and talking to scientists at a conversational level do not get a true picture of the high pressures active in the research scene. When we do, many of us decide that we would rather live at lower tension levels or in other pressure areas. Conant has many more

pungent remarks to make about the fallacy of Karl Pearson's diagram of the scientist as a neutral observer and the study of sciences as the best education for young men (15).

Therefore, to put the scientist on a pedestal because he is an impartial inquirer is to misunderstand the situation entirely [p. 9].

To say that all impartial and accurate analyses of facts are examples of the scientific method is to add confusion beyond measure to the problems of understanding science. To claim that the study of science is the best education for young men who aspire to become impartial analysts of human affairs is to put forward a very dubious educational hypothesis at best [p. 10].

I quote Mr. Conant and others to illustrate the human elements in research and to show that these crucial dynamics are the proper concern of physicians and psychiatrists as well as of the natural and social scientists. The formal descriptions of both discovery and experiment have discouraged in the past, and are now intimidating many clinicians who might be interested in doing investigative work if they better appreciated the early stages of our efforts and the high value of good empirical work. Many men cannot believe very enthusiastically in the value of the single-case study, no matter how well done, if organized agencies do not give prestige and rewards for this work.

A. N. Whitehead comments on this situation in his book, *Adventures in Ideas* (58):

Modern scholarship and modern science reproduce the same limitations as dominated the bygone Hellenistic epoch, and the bygone Scholastic epoch. They canalize thought and observation within predetermined limits, based upon inadequate metaphysical assumptions dogmatically assumed. The modern assumptions differ from older assumptions, not wholly for the better. They exclude from rationalistic thought more of the final values of existence. The intimate timidity of professionalized scholarship circumscribes reason by reducing its topics to triviality, for example, to bare sense and to tautologies [p. 151]. . . . It is one task of speculation to urge observation beyond the boundaries of its delusive completeness, and to urge the doctrines of science beyond their delusive air of finality [p. 199]. . . . Nature is patient of interpretation in terms of Laws which happen to interest us [p. 174]. . . . The criticism of theory does not start from the question, true or false? It consists in noting its scope of useful application and its failure beyond that scope. . . . Some of its terms embody a general notion with a mistaken specialization, and others of

its terms are too general and require discrimination of their possibilities of specialization [p. 285].

The method of teaching endorsed by Mr. Conant throws light upon our problems also (15).

A few words may be in order as to the principles which would guide me in selecting case histories for my hypothetical course in the Tactics and Strategy of Science. I should wish to show the difficulties which attend each new push forward in the advance of science, and the importance of new techniques: how they arise, are improved, and often revolutionize a field of inquiry. . . . I should have very little to say about the classification of facts, unless it were to use this phrase as a straw man. But I should hope that almost all examples chosen would show the hazards which nature puts in the way of those who would examine the facts impartially and classify them accurately. The "controlled experiment" and the planned or controlled observation would be in the forefront of every discussion. The difference in methods between the observational sciences of astronomy, geology, systematic biology on the one hand, and the experimental sciences of physics, chemistry, and experimental biology on the other should be emphasized [pp. 18-19]. . . . However, I cannot emphasize too often that the course in question must *not* be concerned with the fruits of scientific inquiries, either as embodied in scientific laws or theories or cosmologies, or in the applications of science to industry or agriculture or medicine. Rather, the instructor would center his attention on the ways in which these fruits have been attained [p. 20].

More experimental psychologists are also favoring a less ambitious empirical approach in areas where this is the most appropriate procedure. I will cite only a few of many who make a strong, well-reasoned appeal that experimenters work on important problems even though these do not lend themselves at this time to controlled conditions; that the more modest methods and goals of empiricism be employed for the time being since this would permit the free play of ideas which will probably reward us the most in the long run. Ruth S. Tolman (56), in a delightful essay, points out that some men like Warner Brown (12) and D. W. MacKinnon (34) believe we have too much theory and too little fact while others believe the reverse so that she argues for industry in both. They need not be and probably cannot be exclusive.

In order to teach by means of the case history method much more preparatory work needs to be done. We have tried over a



period of years to read classics in psychiatry with full attention to the biography of the author in his historical, intellectual, social, and economic setting, but we need more intimate information about most of our leading writers even of recent times. Kraepelin, Wagner-Jauregg, Janet, Rorschach, and Kretschmer, for example, left little personal material and this is supplemented by a limited amount of descriptive material from students. There is much more material available about Freud, Jung, Adler, Ferenczi, and Abraham. With the publication of Freud's letters to Fliess(22) and Ernest Jones's magnificent biography(28), we can probably do a much better job here if we really want to. The essays by Bernfeld(4) and Erikson's study of the Irma dream(19) are of considerable significance to understanding Freud's work. Perhaps a number of investigators now living could give us biographies of Adolf Meyer, Paul Schilder, and Harry Stack Sullivan which would go deeper than the usual academic life stories. We might well hope that some investigators now living would write out their own life experiences, especially as they relate to the work of discovery and experiment, when it becomes generally understood how important such material will be to future generations.

A fourth method of teaching something about observation and experiment would be to focus in a small discussion group on a problem such as symptom formation in hysteria, the meaning of delusions and hallucinations, or the evidence for the existence of active unconscious processes with comparison of how different men engaged themselves with these problems. The historical material in such standard histories as Boring's *History of Experimental Psychology*(7) or Zilboorg's *History of Medical Psychology*(62) could furnish guides to more intensive reading. Similarly, small discussion groups utilizing such survey articles as "Animal Research and Psychiatric Theory," by Frank Beach(2), or "Psychosomatic Problems; Methodology, Research Material and Concepts," by Paul Hoch(27), would help residents to orient themselves to current methods. Everyone will have his own preferences as to which published experiments seem to him to be worth studying

and since our field is very large no attempt will be made here to indicate comparative excellence. The major emphasis should be on how the experimentalist went about his job and not on the end results. For beginners in the field of psychosomatic medicine I would stress the value of those experiments which correlate psychological patterns with the simplest physiological functions, the simpler the better.

I have had no recent experience with laboratory courses in psychology or psychiatry. There are many texts in the former field. Perhaps similar courses might be tried in psychiatric residencies with real benefit, but most teachers I know prefer to spend their time on the supervision of the daily clinical questions of residents which is, of course, also a laboratory. Perhaps we need more leisure and maturation before we can set up laboratory exercises in psychiatry which are worth the effort. Certainly most of the older demonstrations in hypnosis, animal maze running, conditioning experiments, nonsense syllable learning, association-motor or galvanic skin reflex techniques, etc., will not satisfy the needs of a man now in residency training, but it seems to me that the newer conditioning, perception, and learning experiments might be of genuine interest (Liddell, Horsley, Gantt, Cantril, Klein, Holt, *et al.*).

As stated from the beginning, however, formal discussion about scientific method, valuable as it may be to experts at appropriate times is not the most essential aspect of the teaching. Students should know that there are many approaches possible and be given a chance to see them in action, as it were, but this will not alter the basic difficulties between a man and his medium. That research can be categorized into such subdivisions as descriptive, historical, experimental, philosophical, prognostic, sociological, and creative, and that each has its own conventions does not loom large in the overall picture(61). I believe that as a matter of general interest and orientation residents should read the outstanding essays by such leaders as A. N. Whitehead(58, 59, 60), Bertrand Russell(49), Max Planck(41), Albert Einstein(50), P. W. Bridgman(10, 11), Sir Charles Sherrington(51), J. R.

Oppenheimer(40), and Niels Bohr(6, 18). The mathematicians and physicists have much to offer us in methods, perspectives, and wisdom. Their imaginativeness and tolerance are an inspiration for those who are laboring in different media with much less advanced methods. Their courage in solving extraordinary dilemmas furnishes hope that we also may make better sense out of man in action.

Before closing, I would like to mention a number of books which are not easily classified since they range over a wide variety of formal, informal, and even familiar discussions of the investigator and his work. I would like to recommend these books highly since I have derived both pleasure and understanding from them. Although they are not as searching in the personal sense as the work of Kubie and Roe reviewed earlier, they do provide insights which are valuable to many workers. There is no effort here at completeness and many more might be added, but I choose those which emphasize the human side of the experimenter's work. Foremost among these is Claude Bernard's *Introduction to the Study of Experimental Medicine*(3), first published in 1865, which is known to you and needs no praise from me. On re-reading it, I find many useful discussions which point up questions in psychiatric research. Another worthwhile classic is *The Art of Thought* by Graham Wallas(57). The section on "The Scientist and Science" in Henry Poincaré's *Science and Method*(42) has some sound insights into the subjective aspects of his work. Two more old friends are Cannon's *The Way of an Investigator: a Scientist's Experiences in Medical Research*(14) and Ramón y Cajal's *Precepts and Counsels on Scientific Investigation*(44) (whose subtitle is "Stimulants of the Spirit") which help us in our efforts to bridge the gap. Two other books with much subjective material about the investigator are those by W. I. B. Beveridge on *The Art of Scientific Investigation*(5) and *The Scientist in Action* by W. H. George(23). The latter promises more than it delivers but has these thoughts in the preface.

This book is about science as action—as just one type of human action, practiced by a few men, and

by them, only upon occasion. No matter how strange, fantastic, unnatural, and "inhuman" such action may seem, its achievements leave no doubt of the tremendous human potentialities when, as in science, all questions of "why?", of creed, of values, or of nationality are completely ignored.

Studies of scientific method are common enough, but in this book the treatment is strictly *scientific*. . . . In abandoning traditional views, scientific research is here treated as a problem in human action. The very basis of science—the observable facts—is seen as a certain type of human experience, and the scientist himself becomes a part, not only of his apparatus, but of his results also. . . . To talk about science without talking also about the scientist is rapidly becoming meaningless [p. 9].

As a last recommendation, I like that article by W. F. G. Swann on *The Nature of Research in Physics*(54). With delightful humor he examines a number of the more personal aspects of research in a laboratory using concrete illustrations of the patterns of motivation and behavior under laboratory stress. The psychiatrist with a little practice would be as much at home interpreting these action patterns as he would in the clinic. His summary at the end of his paper may be of interest to us in our search for likely young men.

And so, to sum up this rather long speech, if you should ask me what I deem to be the characteristics of the successful researcher, I would say that he is a mixture of characteristics which do not always seem consistent one with the other. And yet, of course, in the last analysis, there is consistency and only in a superficial sense is there inconsistency. Our researcher should preferably be endowed with a strong desire to find new truths of nature. In a slightly lower plain, he should enjoy the battle of overcoming difficulties. He should plan his researches in terms of units of accomplishment rather than in terms of time spent. He should develop the characteristic of working under pressure from within. Sometimes he should be highly logical in analysing his problem to the extent that efficiency dictates by rigorous mathematical procedures. Sometimes he should dismiss logic in the superficial sense and simply try things or even proceed by rule of thumb. However, in this connection, the temptation to potter is very great and the researcher should resist that temptation and keep his eye continually on the main goal. The researcher should try and develop in himself, through proper appreciations of orders of magnitude and similar knowledge, the same kind of sensitivity to the phenomena of nature which he is investigating as he has in dealings with the things of everyday life. The successful researcher must possess the power to stick at a thing until he has given whatever procedures he is adopting a reasonably fair chance of success. However, he must have the



courage occasionally to make a complete break, in the sense that he alters completely his attack upon the problem. This is perhaps one of the hardest things to do; for if one has spent months along certain lines and is asked to discard all the work he has done for a different method of approach, he cannot avoid a feeling of depression. There is comfort, however, in the fact that frequently the time apparently lost has not in reality been wasted, but has served to bring into more clear focus than otherwise would have been possible the merits of the final procedure which has been decided upon.

The true researcher, by his very nature, cannot live continually in the realms of mental bliss. Sometimes he is very happy, sometimes he is very depressed. If one should plot his happiness against the time, with the horizontal axis representing zero happiness, with points above representing positive happiness and points below, unhappiness, one would obtain a curve with many peaks above and below the axis. In many callings one's curve lies a little above the axis, and has few hills in it. For my own part, I would rather be sentenced occasionally to the dungeons of despair, if, by this means, I could occasionally be allowed to see the mountains, than I would live on the smooth plain where there are no mountains, no crevasses, and nothing of interest in sight fore or aft, and where one was continually conscious of the fact that the next most important thing in his life would be his funeral [pp. 43-44].

I do not know how these young men will call themselves to our attention, but I hope we can recognize them and help them when they arrive.

#### SUMMARY

1. Research in psychiatry as in other disciplines is a vital necessity, not a luxury.
2. Psychiatrists have several functions in improving research: actual participation, active and passive support, and the study of investigators. The enormous importance of unconscious motivation to work patterns is now better appreciated.
3. In order to improve discovery and experimentation, we need to know more about it as an art, as a science, and as an industry. The human elements in research are only now becoming the object of serious study.
4. Science as "empirically reduced empiricism" (Conant) is held to be the most advantageous strategy at this time.
5. The ways in which men really work at experimentation is both a worthy and a possible object of study. The case history method is cited as a means of acquainting those without practical experience of the hazards of research.

6. The need for long-term studies is emphasized. Many psychiatrists can make a personal contribution by writing of their own struggles, and also by careful reporting of their experience with professional investigators.

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## DISCUSSION

JOHN ROMANO, M.D., Rochester, N. Y.—I read Dr. Brosin's first draft and listened today to his talk with interest and appreciation. Certainly there is little room for disagreement with his exposition of needs and objectives for fundamental research in psychiatry. Whether the oft-quoted figure of 9 million patients in the United States will prove to be valid is questionable in my opinion. Currently, case finding techniques and reporting are hardly precise enough to support the quoted figures, with the notable exception of patients committed to institutions. The quoting of such figures indicating magnitude and urgency may serve as constructive propaganda to alert and to enlighten administrative government officials and foundation directors in determining how and where public and private monies may be diverted to psychiatric research. It may also serve to inform all citizens of the nature of the problem. I question, however, if awareness of either the magnitude or the urgency of the problem necessarily initiates or supports incentive for those interested in pursuing new knowledge. In my opinion, the capacity or talent for research is a deeply personal quality, one external manifestation of which is usually called curiosity, which may be initiated by many factors,

subtle and gross, and often unrelated to the reality needs of the problem pursued.

It is generally agreed that there are equally imperative needs for research in medical fields other than in psychiatry. It is assumed that more fundamental research is taking place in the other clinical fields by a greater number of talented and seasoned researchers. Whether this is actually so I cannot say. However, impressions obtained from personal and random observations in 6 university medical centers in the past 20 years has not convinced me that any clinical medical discipline can boast of a surfeit of unusually creative investigative minds. All fields must compete for the creative mind, as they compete today for financial support from public and private resources.

In general, early in this century, psychiatric research was conducted by individuals, usually unaided by grants. The generosity of the American foundations to psychiatric research in the second quarter of this century is well known and generally appreciated by the psychiatric profession. In the past 9 years since the Seventy-ninth Congress passed the National Mental Health Act, the federal government through the United States Public Health Service has contributed meaningfully to the support of psychiatric research as well as to training programs.

Even though the record of achievement is notable in many respects, there remains the impression that there has been a lag in psychiatric research as compared with the pursuit of new knowledge in other clinical medical fields. Whether this is so when viewed in proper historical perspective is a question which can only be answered later. The conventional apology for the lag is usually composed of the following factors: the complexities of the study of biology in human as contrasted with mammalian concepts; multiple causality in multiple-person fields of action; the great difficulty in dealing with segments or parts of human behavior; the inevitable time limitations in studying human growth and development; the immediate and primary concern of therapy and the healing art in the urgent need to care for the sick; the lack of facilities for research; the lack of skilled investigators and extraordinarily limited funds for this purpose. By no means is this an exhaustive or complete enumeration of the factors often presented as reasons for the lag. It is also well known that there are very few persons engaged in investigative work who may serve as models with whom the young career investigator may identify. The psychoanalytic institutes, with a few exceptions, have conducted themselves more like the postgraduate didactic medical schools of the early part of this century. In these, more attention has been directed towards the communication of existing knowledge and skills as they relate to the healing art and technical craftsmanship rather than to critical scrutiny of existing data and to the pursuit of new knowledge.

It is also well known that there has been an increase in the number of new departments of psychiatry in medical schools which have become major departments in such centers. However, these

at present are still few in number. They are young in department age and have had necessarily to devote energies initially to organizational, clinical service, administrative, and teaching areas.

I believe another significant factor in the development of psychiatric research in this country relates to the fact that the United States Public Health Service grants for research necessarily through Congressional mandate have had to be based on project research. In my opinion the officers of the United States Public Health Service through the National Institute of Mental Health and the many civilian consultants who have participated in the recommendation of allocations for project research deserve high tribute from all of us for their devoted, perceptive, and intelligent discharge of their assignments. However, the fact remains that as yet it has not been possible to provide the use of federal money for research purposes other than through the medium of *ad hoc* project research.

It is heartening to learn of the development of a more liberal and farseeing policy in various foundations leading to grants for unrestricted research interests and for career fellowships. The 5-year-Markle Foundation scholarship in medical science is an excellent example of critical selection of potential leadership in medical science. The recent establishment of career fellowships from the American Heart Association and more recently in psychiatry by the United States Public Health Service are further examples. The Foundation Fund for Research in Psychiatry has also adopted long-term policies similar to those mentioned. It would be my firm hope that it may be possible for those who allocate money, both public and private, to return to some of the principles advocated and followed many years ago by that wise man who had so much to do with the renaissance of medical education in our nation, namely, Abraham Flexner, whose policy it was to select men critically, to bet on them so to speak, to give them what they needed to do their work, and then to let them alone.

There is need, I believe, for unrestricted money, money that can be used to subsidize research scholars who would be able to work on problems at preproject level, that is, to pursue knowledge for its own sake, the relevance of which would be determined by the investigator and the university department and not by a foundation or other source of money. I have no hesitation in saying that the university department has a serious responsibility in establishing a climate of opportunity for the person who wishes to work on problems. I believe this climate of opportunity requires models of research scientists with whom others can work and become identified. I disagree with the oft-quoted statement that the opportunity for research in this field should be restricted only to seasoned clinical veterans. I feel the opportunity should be open to persons at all levels of sophistication and experience with proper attention to the safety of the patient or human subject. I don't believe that one can enforce or command research interests. I believe that the university department must be able to create opportunities for those who wish to pursue

their interests under the guidance and assistance of more experienced persons. Currently there is a feeling that psychiatric research must necessarily be team work necessitating a constant intercommunication between persons with different perceptions and experiences. I feel at times that this movement has become somewhat evangelistic. My own feeling is that some persons may wish to work alone for brief or extended periods. Others may feel more comfortable and may be able to be more creative when they share their experiences and ideas with others of similar beliefs or with very different beliefs and experiences. I am sure that the success of any multidisciplinary group effort depends to a great degree on its spontaneity and natural growth rather than on a mandatory table of organization. I believe that research results from highly personal internal psychic pressures and is not necessarily altruistic. It has something to do with curiosity, with persistence, with tenacity, with imaginativeness, with discipline; it is multi-motivated and variously manifested. I hope sincerely that it may be possible to persuade those who allocate money to do so not only for project or program research but to allow persons at various levels of experience to pursue their ideas as they wish. It is important, too, that those responsible for leadership be perceptive and alert to the unusual creative minds who come to their attention. Every opportunity should be taken to stimulate and interest them, to foster and to help them in their search for the solution of ideas and problems in their minds.

Recently the National Institute of Mental Health has been established as a research institute. There has also been discussion as to the possibility of establishing special types of research institutes in psychiatry. The following statement made by me previously relates specifically to consideration of an institute designed for research in the field of psychoanalytic psychology. However the statement also deals generally with the controversial problem of the special institute as contrasted with the university, and I shall take the liberty of quoting from my statement in order to explain my position in this matter: *I am in full agreement with the principles that psychoanalytic psychology, both through method and substance, has introduced operational hypotheses which have made possible a better understanding of human growth and development, of health and disease, and of personality function. I am also in agreement that I know of no alternative method or body of knowledge which is currently as useful and as promising in the study of human behavior. If we assume that the staff or faculty of such a special institute be recruited from those persons who have acquired advanced scholarship in the method and knowledge of psychoanalytic psychology, will this restrict or narrow their perception of human behavior? Will such a group be concerned principally with logical extensions of existing knowledge? There is no question in my mind that this would be very profitable and very useful. In other words, the group would share in common the acceptance of certain working hypotheses, would attempt to verify ob-*

*jectively, and in doing so would attempt to obtain greater precision of knowledge and predictive value. Is it possible that such a staff with a common set of concepts and limited clinical material will find it more difficult to perceive human behavior differently than in the way in which they have been trained and with which they have experience? It appears to me that opportunity should be afforded, junior persons particularly, to observe human behavior without necessarily having the same conceptual framework of their seniors. From such observations, it may be possible to experience what has been called the inductive leap, to create new concepts as yet unintelligible in terms of the classical theory of one or other existing science or thought disciplines. I am not convinced that the university department may necessarily provide such an opportunity, but I believe that it may be better able to do so in terms of the younger scientist and in terms of the broader clinical experience which is afforded him.*

In my opinion, progress in psychiatric research will result from the establishment of opportunity and adequate facilities for the creative minds among us, now and in the future. We shouldn't become too impatient. In the past hundred years there have been only 5 persons, Charles Darwin, Louis Pasteur, James Clerk Maxwell, Albert Einstein, and Sigmund Freud who have been able to make the inductive leap.

## DISCUSSION

JACOB E. FINESINGER, M.D., Baltimore, Md.—It has been a pleasure to read and to hear Dr. Brosin's paper. This is a scholarly and thought-provoking contribution in a current and vital area. Dr. Brosin with his usual thoroughness has discussed the personal, sociological, and many other factors which bear upon discovery and experiment in psychiatry. In discussing this paper, I should like to mention some impressions of the current residency program, comment briefly upon a few of the psychological factors most likely at work in the investigator. I shall describe briefly some of our teaching attempts in dealing with the problem Dr. Brosin has presented.

There is no lack of agreement that research is needed in psychiatry and its allied fields. Years ago we believed that our research effort could expand adequately if the necessary funds and personnel were available. In recent years funds for research and for training have become available, and yet many of us close to the scene have been disturbed. The number of young men and women going into psychiatry has greatly increased. The influence of psychiatric and dynamic concepts is felt in many departments in the medical and in allied professions. Yet, many of us, including Dr. Brosin, I believe, feel disturbed at what appears to be a lag in the rate of progress in research—in opening up new frontiers, in developing new theories, or in establishing the validity of existing theories. I agree with Dr. Brosin that we see comparatively little research interest on the part of our residents. There is great interest in learning to



treat patients, and in fitting clinical phenomena into psychodynamic theory. I have been impressed during the last decade with the persistent need on the part of residents and young psychiatrists for following closely to the well-defined theoretical line and a hesitation to consider alternate hypotheses to explain the complex behavior of patients. It seems that our training has misfired in some way. To be sure, we have been able to develop in our residents a sensitivity in recognizing the subtle cues given by patients and the subtleties of transference and countertransference phenomena. Yet I fear that the zeal for conforming to theory leads to a lack of boldness in re-examining the raw data closely and critically in the hopes of achieving still more fruitful insights. I sense a reluctance to grapple with the nonverbal and verbal details of clinical material, except in strict conformity with current psychodynamic theory. It is as though our training has put too high a premium on the acceptance of theory—as though it has libidized theory at the cost of freedom of thinking. The libidization of even a productive theory can strike right at the roots a process which must not be blocked if we are to do meaningful exploration and research in psychiatry.

It is essential that our young psychiatrists have psychoanalytic training. This training they must acquire in institutes, usually outside of the university and differing from the university in history, traditions and values. This difference may play a role in our current dilemma regarding research. The fact that the training analyst without academic support can afford little time for research tends, I believe, to allow the student to identify with a model whose work is largely teaching and practice. It is hardly necessary to point out the importance of identification in the subsequent career of the student. This problem may be largely resolved when training in psychoanalysis becomes a university function and when accreditation is entrusted to an independent organization.

Our plight in psychiatry may be no worse than in medicine or surgery, or for that matter, in other technical schools. It may well be that the motivation of those who go to medical school is primarily to practice and that the potential investigator becomes weary of the long arduous training of the physician topped by additional years of training in psychiatry and psychoanalysis. Dr. Brosin has referred to the economic problems of the young psychiatrist and his involvement in other commitments, which direct his professional course in ways not conducive to productivity in research. But clear it is that if we are to play a part in resolving this dilemma, work needs to be done in studying these manifold problems and in evaluating our teaching and training in this respect.

To understand how new knowledge comes about, we cannot limit our concern to any one segment of the investigator's career. The motivation, the special gifts and talents necessary for a successful and productive career in research must have roots in earlier experiences. The work of Dr. Anne Roe and the interesting paper of Dr. Kubie are

beginnings in understanding this area. If time permitted, I should like to tell you of a patient I had in analysis. This patient was a physical anthropologist who persisted in his interest even though his field was being supplanted by the more modern branches of anthropology. My work with him taught me much about at least one investigator. In trying to unscramble the many events in his past life which could be related to his motivation, I would have to give high priority to some early experiences which seemed meaningful. This patient's father was a sculptor, and he recalled being terrified of the torsos and unfinished sculptures in his father's studio. There were recurrent anxiety dreams of statues without heads and arms. As a child he would bury dead animals and after a time dig them up to see if the skeletal bones were still there. This activity was a dominant interest in his life at this time—and he recalls the feeling of relief and accomplishment in finally setting up the skeleton of a cat or a rabbit. I shall spare you the details of the free associations which related this material on the one hand to personal anxiety and conventional castration material, and on the other hand to his motivation and specific choice of research activity. Does the motivation of every researcher, successful or not, represent an attempt at dealing with repressed anxiety—as this clinical material seems to indicate? I would not know the answer. We do know that many of the activities of the researcher stir anxiety or at least tension. Waiting for the experimental results to come in, pushing for the data to disprove one's own pet hypotheses; these require the capacity to take tension. I could cite numerous examples from our teaching experience to illustrate how forcing the student to broaden his horizon, to cut through the barriers which segregate one discipline from another, produced tension and anxiety which in some instances disrupted the learning process. Yet some degree of tension may be necessary, for as we all know, there exist social institutions whose function seems to be to effect closure, to offer certainty, and by so doing dampen the bold inquisitive spirit of man.

It would seem that in the psychological development of the individual and his motivation there are many facets which influence the choice of special areas of investigation and the tools or processes used. I would believe it possible that the fixation in any or several of these experiences may account for the special meanings that research has for different investigators. Some are concerned with theoretical hair-splitting; to others, it means belonging to a selected fraternity of dedicated individuals; and to still others, it may offer another administrative opportunity of getting involved in the business of research.

When some of these activities become the major goal, whether the researcher is aware of this or not, the probabilities are not great for contributions to the field. Occasionally advances are made in spite of the confusion in goal—occasionally fortuitous circumstances come to the rescue, and

some ideas and material can be salvaged by the rescue squad.

The fact that many of the traits and actions of the investigator show roots deeply buried in earlier experience does not necessarily imply that they cannot be modified. Perhaps certain traits such as persistence, the capacity to stand tension, can be kept from being disruptive only when the personal support of a teacher or a colleague is available. I have seen—and I am sure that many of you have had similar experiences—students and younger colleagues learn to cut through existing barriers in thought and fashion, and make new and productive associations. Occasionally one sees younger colleagues learn to get closer to the more fundamental aspects of a problem and carry over the same shrewdness and sensitivity to other problems. It may well be that people like Freud and Einstein need little help from others. Yet as Dr. Brosin mentions, we don't see many such and, hence, feel justified in pursuing teaching and training programs to develop and support gifted investigators.

So the problem remains—what can we do as teachers and investigators to stimulate attitudes, interest, and skill in study and working which may result in further discovery and experiment in our field? Whatever we do must take into account the human dynamics and the best available information in these areas.

I would agree that the conventional didactic methods of training medical students have not stimulated a large scale production in research. Our point of attack can well be earlier than that of the residency in psychiatry. Ideally we would like to begin even before formal schooling to do everything possible to foster certain attitudes and an orientation to the world we live in. The first real opportunity is in the selection of medical students. Here one feels the great need for research in screening devices to help select those students specially gifted in flexibility in thinking. Our selection devices, I believe, would be more effective if they could be more operational and rely on the actual performance of a candidate in solving problems closely akin to the work of the investigator.

We are concerned with fostering certain attitudes—attitudes toward people and toward the subject matter. The medical student obviously needs information to be a doctor. Yet the acquisition of sound information is not the only goal. It might be preferable to put the emphasis primarily elsewhere, on trying to understand how science comes about and on the process of obtaining and validating fact. Since the doctor works with human beings, human values—personal as well as social values—become vital. In order to deal with the multidimensional factors in disease, it is necessary for most students, trained as they are at present, to break down the interdisciplinary barriers even though they are man made. A goal in this area would be to help the student move back and forth from the biochemical and physiological to the psychological and sociological levels. We would

prefer to have the student navigate with equal competence and security in all of the content areas important in medicine. In considering any one area of subject matter, one of the objectives is to achieve flexibility in vertical locomotion so that the student can move up and down from observation to theory, to presupposition, to hunch. We need to create situations of high interest in the medical school which would enable the student to speculate beyond the fixed borders of knowledge and allow him to use his imagination to explain what he knows and to speculate in the areas which he, and for that matter no one else, knows. We would allow the set to force the use of imagination—the important requirement being that the student know at what level the material he is dealing with happens to be. Such an approach, I believe, would in some measure counteract the trends in current scientific thought which Whitehead decries. The role of teachers of various disciplines from philosophy to biochemistry becomes apparent if we consider our job to be more than the teaching of "facts." Perhaps we may well consider ways and means for allowing the student to acquire experience not only in the accurate observation of the phenomena but in the ground rules of theory building, based on these observations. This is indeed a tall order, and the problem is how to achieve this in the present social structure of the medical school, with its understandable emphasis on information and practical skills. I have often felt that a great deal of the time of the medical student could well be spent in acquiring the attitudes and the operational skill needed to struggle with problems in these broad areas of speculation.

But is there not a danger in this emphasis on theoretical areas, and would this not interfere with the more conventional goal of seeing to it that the medical student obtain sound information? I do not believe that these are exclusive. The medical school and the hospital offer ideal opportunities in the study of patients; this subject matter is of importance and has high interest value to the student. Clinical observations are empirical, and can be used as the raw material from which speculation flows. There is always the danger that speculation for the sake of speculation becomes an end in itself. This type of activity has some value, to be sure, in forcing adherence to logic and developing verbal and intellectual facility. I believe that the student has a different type of experience when speculation becomes a means toward solving the clinical problem presented by the patient.

We are attempting as best we can to carry out this type of teaching and training in undergraduate work with medical students and in graduate work with residents. A patient or the recorded material from a patient is observed by a large or small group of students. It is important that all involved have the opportunity of observing and studying the same material. The problem is always to explain a particular phenomenon as completely as possible, and to recognize and explore the un-



resolved problems to the limit. The role of the teacher is that of a catalyst, rarely a resource person. He uses minimal activity and intervenes only when the process of inquiry is halted or bogs down. The nature of the problem and the nature of the responsibility change with the state of the student's development. The resident is concerned with more complicated issues, and the young investigator may become involved in a research problem of his own choosing—but the method of operation and the role of the teacher are essentially the same. As the student progresses the teacher can withdraw more and more and assume the role of a colleague.

The suggestions that Dr. Brosin makes of reading the classics in psychiatry with full attention to the biography of the author and the setting,

the use of the case history method, the development of laboratory courses, the use of courses in statistics and in research design and methods, and the broad reading all are worthy and should be explored and evaluated. To me, they do not supplant the type of learning experience I have tried to describe. The purpose is basically to enable the student to become involved and directly participate in an experience of high interest, to scrutinize the experience, draw inferences from the experience, to try to explain it, to use his imagination to the hilt in speculating about it, and then if possible to generalize about it. I believe that the meaningful learning which often occurs in these teaching situations offers one approach to discovery and experiment.

I should like to thank Dr. Brosin for this opportunity to discuss his clear and thoughtful paper.

## FACTORS INFLUENCING DISCHARGE OF FEMALE PATIENTS FROM A STATE MENTAL HOSPITAL<sup>1</sup>

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Many studies on the effect of treatment and the prognosis of mental disease in state hospitals have been published. In the older literature the argument as to whether or not mental disease was curable was widely discussed in medical journals. Parsons(1) in 1884 and others recognized that discharge of patients was no criterion of recovery. He pointed out that quite ill patients were at times discharged while many who seemed well were not. In 1938, Romano and Ebaugh and, in 1939, Malamud and Render surveyed the prognosis of patients diagnosed as having one disease, schizophrenia. In one of these studies the authors (2) ignore the problem of discharge *vs.* improvement, and all discharged patients are scored as improved and all remaining in the hospital unimproved. In the other(3) the authors note the discrepancy between improvement and discharge but did not publish their tabulation.

With the increased emphasis on active treatment in state hospitals and with the mounting statistics on the effectiveness of lobotomy in particular, it seems increasingly important to study the factors that influence the discharge of patients. Though discharge has relationship to recovery, obviously it is not equivalent to it. This paper will seek to determine which of a number of factors surveyed most influenced discharge, readmission, and, to a lesser extent, improvement.

### METHOD

As indicated in the title, only female patients were used and, for practical reasons, only white patients. Two hundred and six of approximately 800 such patients available were selected primarily on the basis of their responsiveness, positively or negatively, to friendly greeting on the ward. If, after brief interview, it was felt that a patient possessed

personality assets that could be sufficient for rehabilitation and discharge, Binet Vocabulary and cards I, III, and VIII of the Rorschach were administered and recorded. These findings were collated and a score of the patient's "treatability"—which in the charts is called "ego"—was made on a six point scale, 0—5. Zero indicates absence of defect and 5 maximally defective. The following were also scored on a 6-point scale: age, length of stay (at the time of screening), skill, education, and family status (Table I). Diagnosis as made by hospital staff was noted but was not used as a scoring criterion.

Formal treatment of various types (to be described later) was given to certain patients, and at the end of 2 years discharges and readmissions were noted, and most of the patients in the study group remaining in the hospital were re-evaluated as to "ego" rating. The results were tabulated and subjected to statistical analysis. Since there was little social work with patients' families (10 to 15 cases were served briefly), this study in a way represents a study of the "natural history" of patients in a state hospital.

### RESULTS

Of the 206 patients under study, 66 or 32% were discharged at some period during the 2 years. Twenty-five or 38% of those discharged were readmitted, and, of these, 6 or 24% were redischarged. The number classed as discharged at the end of the study was 47 or 23% of the total group. Since our interest lay in what factors facilitated discharge, we shall focus our attention on the 66 primary discharges and take up secondarily the readmissions.

The relationship of the scored variables to discharge are found in Table 2 and to readmissions in Table 3. Since the numbers are small, it was found that the results were more meaningful if data were grouped to

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TABLE 1  
CATEGORICAL EVALUATION OF PATIENTS

SCORE	EGO	AGE	LENGTH OF STAY	EDUCATION	SKILL	FAMILY
0	NO OBSERVED DEFECT	20-29	LESS THAN ONE YEAR	PROFESSIONAL	PROFESSIONAL	MARRIED WITH LIVING CHILDREN
1	NEUROTIC DEFENSES ONLY	30-39	1-2 YEARS	COLLEGE	SKILLED WORKER	HAVE BEEN MARRIED AND BORNE CHILDREN
2	EGO IMPAIRMENT OBSERVABLE	40-49	3-4 YEARS	HIGH SCHOOL GRADUATE	DOMESTIC SKILLS (COMPETENT)	WIDOWED WITH LIVING CHILDREN
3	RESPONSIVE WITH DEFINITE EGO DEFECT	50-59	5-9 YEARS	SIXTH GRADE TO HIGH SCHOOL	ARTISTIC SKILLS ONLY	MARRIED NO CHILDREN
4	MARKED EGO IMPAIRMENT	60-69	10-19 YEARS	UNDER SIXTH GRADE	POOR WORKER	WIDOWED NO CHILDREN
5	DETERIORATED	70 AND OVER	20 YEARS AND OVER	ILLITERATE	NEVER EMPLOYED	SINGLE

TABLE 2  
RELATION OF DIAGNOSIS AND RATING SCORES FOR SIX CRITERIA TO DISCHARGE

SCORE	EGO			AGE			LENGTH OF STAY			EDUCATION			SKILL			FAMILY			DIAGNOSIS <sup>(1)</sup>		
	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL	DISCH.	UN- DISCH.	TOTAL
0	0	0	0	18	16	34	48	21	69	1	8	9	4	14	18	30	42	72	29	81	110
1	5	6	11	11	27	38	6	31	37	4	21	25	5	17	22	1	0	1			
2	14	25	39	19	39	58	2	24	26	6	31	37	45	69	114	11	25	36			
3	22	34	56	13	34	47	6	26	32	42	60	102	0	1	1	10	10	20			
4	13	32	45	5	18	23	3	30	33	12	18	30	11	35	46	3	9	12			
5	12	43	55	0	6	6	1	8	9	1	2	3	1	4	5	11	54	65	37	59	96
TOTAL	66	140	206	66	140	206	66	140	206	66	140	206	66	140	206	66	140	206	66	140	206

(1) 0 = SCHIZOPHRENIA 5 = OTHER

TABLE 3  
RELATION OF DIAGNOSIS AND RATING SCORES FOR SIX CRITERIA TO READMISSION  
OF DISCHARGED PATIENTS

SCORE	EGO			AGE			LENGTH OF STAY			EDUCATION			SKILL			FAMILY			DIAGNOSIS <sup>(1)</sup>		
	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL	NOT READM	READM	TOTAL
0	0	0	0	13	5	18	28	20	48	1	0	1	4	0	4	17	13	30	18	11	29
1	5	0	5	8	3	11	5	1	6	2	2	4	2	3	5	1	0	1			
2	11	3	14	10	9	19	1	1	2	3	3	6	31	14	45	7	4	11			
3	8	14	22	9	4	13	5	1	6	30	12	42	0	0	0	6	4	10			
4	10	3	13	1	4	5	2	1	3	5	7	12	4	7	11	2	1	3			
5	7	5	12	0	0	0	0	1	1	0	1	1	0	1	1	8	3	11	23	14	37
TOTAL	41	25	66	41	25	66	41	25	66	41	25	66	41	25	66	41	25	66	41	25	66

(1) 0 = SCHIZOPHRENIA      5 = OTHER

RELATION OF VARIOUS CHARACTERISTICS OF PATIENTS TO RATE  
OF DISCHARGE, TOGETHER WITH 95% CONFIDENCE INTERVALS.

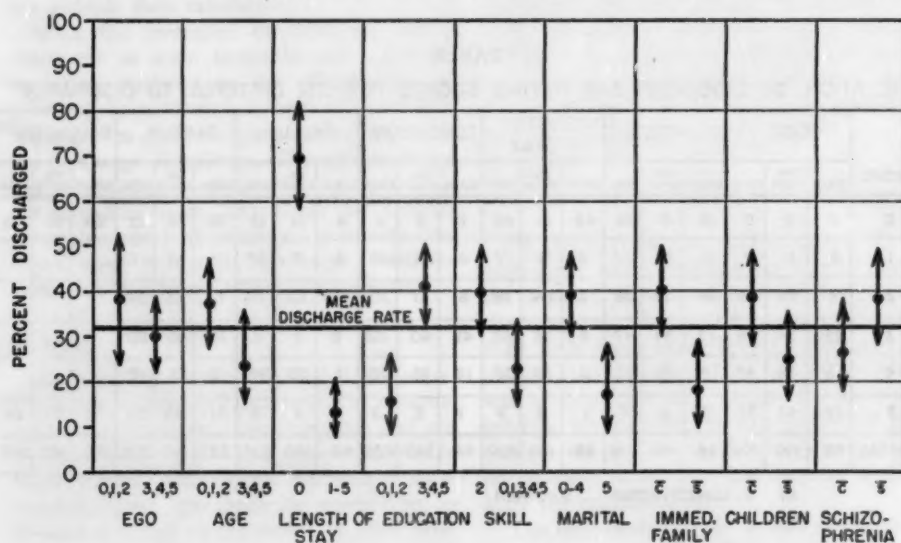


FIG. 1.

form a dichotomy, and comparison made between 2 contrasting categories for each variable. A chart of the relationship of these to discharge is presented showing probable limits of chance variation (Fig. 1). Only one variable (ego) was found to be probably significant in terms of readmission (Fig. 2).

*Ego.*—Though 38% of the better integrated patients (categories I, II) were primary discharges as opposed to 30% of the more deteriorated ones (categories III, IV, V), this difference is not statistically significant. This factor does play a part in readmission rate since only 16% of categories I and II were readmitted compared with 47% of categories III, IV, V. This is probably significant ( $P = .04$ ).

*Influence*

Discharge: Not Significant  
Readmission: Probably Significant

*Age.*—A greater percentage of patients under 50 were discharged (37%) than those in older age groups (24%). However, the difference is slight and is due mainly to an interrelationship between age and length of stay (see below). When data were tabulated according to both age and length of stay, age in itself had no apparent effect on discharge rate.

*Influence*

Discharge: Not Significant  
Readmission: Not Significant

RELATION OF EGO SCORE TO PERCENT READMITTED AFTER DISCHARGE, TOGETHER WITH 95% CONFIDENCE INTERVALS.

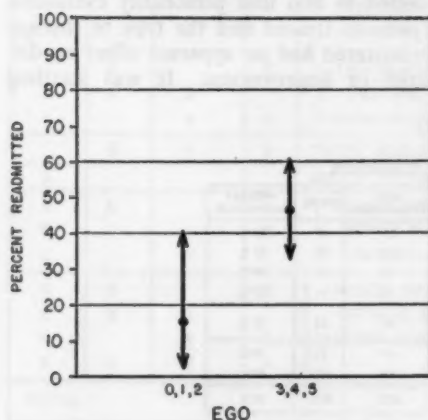


FIG. 2.

*Length of Stay.*—The chance of discharge of a patient who has been in the hospital under 1 year is about 5 times as great as of those who have remained longer (70% to 13%). The probability of discharge falls abruptly after 1 year, and rate changes but little after that time. It should be noted that this category does not take into account the duration of the illness prior to admission to the hospital or the type of onset, abrupt or gradual.

*Influence*

Discharge: Highly Significant  
Readmission: Not Significant

*Education.*—A smaller percentage of patients with high school education or more were discharged than of those with less education. The less well-educated included 3 illiterates, but the bulk of the group had more than sixth grade and less than high school education.

*Influence*

Discharge: Significant  
Readmission: Not Significant

*Skill.*—Only 1 category, the largest, competent at domestic skills (category II), seemed to have any favorable influence on discharge. This effect was found to be apparent rather than real, however, and is related to the more significant variables, education and family status.

*Influence*

Discharge: Not Significant In Itself  
Readmission: Not Significant

*Marital-Maternal.*—For purposes of comparison, the categorical selections in this group were put together in 3 combinations: (1) married as opposed to single patients; (2) those with and those without immediate families; and (3) those with and those without children.

Of the following groupings: patients who were married, had immediate families, or had children, had a better chance of discharge than their opposed pair, the former two significantly so. The effect of having had children on patient discharge rate was less important than the other 2 groupings.

*Influence*

Discharge: Significant  
Readmission: Not Significant



**Schizophrenia.**—Of the 206 patients 110 or 53% carried a staff diagnosis of schizophrenia, and 52 or 25%, manic depressive psychosis. Twelve other diagnostic categories were represented; 23 of the patients were considered "organic," and 9 psychoneurotic. The discharge rate for patients diagnosed schizophrenic was 26%; for manic depressive, 37%; for all others including "organic," 41%. The discharge rate for all patients other than schizophrenic was 39%. The readmission rate after discharge for patients diagnosed schizophrenic was 38%, identical with the total group.

#### Influence

Discharge: Not Significant

Readmission: Not Significant

**Treatment.**—The effect of treatment is found in Table 4. Patients for treatment were unselected as far as the study was concerned. They were chosen primarily by personnel who were unaware of the patient's position on the rating scales. A patient was said to have received individual psychotherapy if she was seen more than 10 times in therapy sessions lasting from 30 to 60 minutes. Psychotherapy groups met 1 to 3 times weekly for periods as long as 2 years. No group contained more than 10 patients. Recreation therapy was carried out by one worker with a group of about 15 patients. In this they were seen several hours 5 days a week in activities designed to stimulate interests: painting, clay modeling, music appreciation classes, dancing, croquet, putting on plays, group singing. This continued for about 2 years. Electroconvulsive therapy was administered to patients who showed mood disturbance only and was administered in the

usual manner 1 to 3 times a week. Though these therapies could not be considered intensive, they had no effect on the discharge rate.

#### Influence

Discharge: Not Significant

### DISCUSSION

When this work was originated, it was designed to study the effect of various treatments on recovery in state hospitals. Since it was the personality of the individual that was to be treated, we expected that the category, ego, would be the most important factor influencing discharge and that the younger age groups would be discharged more readily. Consequently, these categories were at first weighted by multiplying the former by 3 and the latter by 2. A total score was then computed and the best 143 patients (by these criteria) were selected, 72 to be treated and 71 as controls, and the remaining 63 patients who had been scored were excluded. This treatment-control separation proved impracticable because of the constantly shifting ward population and was abandoned. Treatment was given to these and other patients not in the study group. At the end of the 2-year study period, 90 of the patients in the original group who had remained in the hospital and who were available were rescored as to ego. Results are shown in Table 5. The fact that more patients improved than deteriorated was unrelated to formal treatment.

It was surprising to us when the data were collected to find that personality evaluation of patients treated and the type of therapy administered had no apparent effect on discharge or improvement. It was startling

TABLE 4

#### RELATION OF TREATMENT TO DISCHARGE

THERAPY	DISCHARGED	NOT DISCHARGED	TOTAL	PERCENT DISCHARGED
E.C.T.	16	25	41	39 %
GROUP PSYCH.	11	24	35	31 %
RECREATION	6	19	25	24 %
INDIV. PSYCH.	3	8	11	27 %
RECEIVED TWO OR MORE TYPES IN	8	15	23	35 %
TOTAL RECEIVING ANY THERAPY	26	47	73	36 %
TOTAL RECEIVING NO THERAPY	40	93	133	30 %
TOTAL CASES	66	140	206	32 %

01 Cases receiving two or more types of therapy are also listed under each treatment.



TABLE 5

RELATION OF TREATMENT TO CHANGE IN EGO RATING

TREATMENT	CHANGE IN EGO RATING			
	IMPROVED		UNCHANGED	
	NO.	%	NO.	%
TREATED	19	51%	9	24%
UNTREATED	25	47%	18	34%
TOTAL	44		27	

when we examined the 63 patients who, though scored, had been excluded from the study because of poor scores to find that they were discharged at rates identical with the "better" patients. At this point we had the sociological data subjected to statistical analysis, and the foregoing results were ascertained.

It will be seen that there is one highly significant fact which relates to discharge, length of stay, and several others which, though of varying significance, play some part. Length of stay, family status, education and ego were each divided into 2 broad

categories one of which favors discharge, the other mitigates against it, thus:

	Favorable codes	Unfavorable codes
Length of stay.....	0	1-5
Family status.....	0-3	4,5
Education.....	3,4,5	0,1,2
Ego.....	1,2,3	4,5

Rates of discharge of patients according to the presence of 0, 1, 2 or more than 2 unfavorable factors were then determined (Table 6). In this, length of stay (major factor) was treated separately from the other 3 (minor factors). Results are shown graphically in Fig. 3.

Inspection of this shows a gradatim in which probability of discharge varies from  $96 \pm 4.1\%$  for patients with no unfavorable factor to  $7 \pm 3.4\%$  for those who have 3 or more factors, including length of stay, unfavorable. It should be emphasized that these relationships obtain in this study regardless

TABLE 6

DISCHARGE RATES ACCORDING TO PRESENCE OF UNFAVORABLE FACTORS

UNFAVORABLE FACTORS								
LENGTH OF STAY 1-5	FAMILY 4,5	EDUCATION 0,1,2	EGO 4,5		DISCH.	NOT DISCH.	TOTAL	PERCENT DISCH.
				NO UNFAVORABLE FACTOR	22	1	23	96± 4.1%
	X		X	} ONE MINOR FACTOR UNFAVORABLE	3	2	5	} 69± 9.1%
		X			12	4	16	
					3	2	5	
	X		X	} TWO MINOR FACTORS UNFAVORABLE	2	3	5	} 44± 12.4%
	X	X	X		3	4	7	
		X	X		2	2	4	
	X	X	X	THREE MINOR UNFAVORABLE	1	3	4	
X				MAJOR FACTOR UNFAVORABLE	8	18	26	31± 9.1%
X	X		X	} MAJOR AND ONE MINOR FACTOR UNFAVORABLE	1	8	9	} 11± 4.2%
X					4	31	35	
X		X			1	11	12	
X	X		X	} MAJOR AND TWO OR MORE MINOR FACTORS UNFAVORABLE	3	12	15	} 7± 3.4%
X	X	X	X			19	19	
X		X	X			8	8	
X	X	X	X			1	12	
TOTAL					66	140	206	32± 3.2%

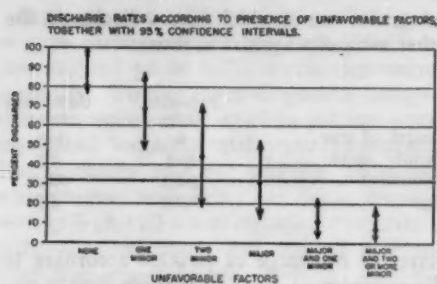


FIG. 3.

of diagnosis and treatment. If these data prove true in other hospitals, they may serve as a base line on which it will be possible to evaluate the effectiveness of therapy or therapies in an institution of this sort.

## SUMMARY

Factors that statistically significantly influence a female patient's discharge from Central State Hospital favorably are (in descending order of significance): (1) Length of stay, (2) *Limited* education, and (3) Possession of immediate family.

Factors, among others, that do *not* significantly influence discharge rate are: (1) Age, (2) Diagnosis, and (3) Treatment.

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## INSULIN COMA THERAPY IN SCHIZOPHRENIA<sup>1</sup>

### A FOURTEEN-YEAR FOLLOW-UP STUDY

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Insulin coma therapy was first employed at the Pennsylvania Hospital on December 8, 1936, and has been in continuous use since that time. This follow-up study was undertaken in order to evaluate its use in schizophrenia, and includes all patients treated through December 31, 1951. In this period, 781 persons were given 903 courses of treatment. This represents approximately 50,000 individual treatments. There were 5 deaths during treatment, and 2 cases of morbidity due to central nervous system injury following prolonged coma. All patients referred by the staff to the insulin therapy unit were private patients, and were accepted for treatment regardless of prognosis, provided there was no medical contraindication. The aim of treatment has been to produce as deep a coma for as long a period as is safe for the patient. Therapy was given 5 days a week, and the average course required 3 months of hospitalization.

The treatment we consider ideal consists of 1½ to 2 hours of coma, of which an hour is in second stage(1), a maximum of 1 hour in third stage, and a maximum of 25 minutes in fourth stage (extensor rigidity of arms and legs). Although our technique has been described elsewhere(2), it should be noted here that prior to 1945 Sakel's technique was followed, and since that time we have employed a rapid increase method of insulin dosage developed by Shurley(2), which shortens the induction phase of the treatment. Electroconvulsive therapy is used in combination with insulin coma if by the thirtieth coma hour the patient has failed to show significant improvement. The ECT is administered when the patient is in second stage coma. The number of treatments is determined by the patient's clinical course. Treatment is terminated 2 weeks after a

stable improvement or recovery has taken place, or after the sixtieth treatment day, by which time the patient will have received 60 to 90 hours of coma, and a minimum of 18 ECT, if combined therapy is given. Insulin treated patients are discharged home if at all possible within a week of the last treatment, and are followed by the referring psychiatrist.

Variation in the diagnostic point of view has been minimal during the period of this study because the diagnoses were made by the same chief of services, Elmer V. Eyman, or the staff under his direction. The diagnostic criteria have closely followed the *A.P.A. Diagnostic and Statistical Manual*, first edition. All but 42 of the 781 treated were diagnosed as schizophrenic. The evaluation of immediate results was made by the hospital staff and insulin therapist at weekly conferences.

Follow-up information was obtained by letter from patients' families, referring physicians, and other hospitals. Because of their generous cooperation, we received data from 85.5% of all patients one year after treatment, from 65% after 5 years, and 60% after 14 years. The data were gathered over a period of 2 years, and were then assembled in groups according to the number of years the patients had been followed. Those treated in 1937 and 1938 form the basis of the 14-year follow-up group, with 54 of the 90 patients heard from; the 12-year follow-up is based on 97 of the 172 patients treated between 1937 and 1940; the 5-year follow-up, on 320 reports out of the 491 treated between 1937 and 1947; the 1-year follow-up, on 668 of the 781 treated between 1937 and 1951. These data are presented in Table 1.

We lost contact with 243 patients before the completion of their full follow-up period; 185 of them were out of hospital, making satisfactory progress and 58 were in hospital at the time of their last report. If we

<sup>1</sup> From the Pennsylvania Hospital, Department for Mental and Nervous Diseases, Philadelphia, Pa. This work has been supported by the Pennsylvania Hospital Division of the Hall-Mercer Hospital.

TABLE 1  
COMPOSITE RESULTS ACCORDING TO PERIOD OF FOLLOW-UP\*

Interval since treatment	Number of patients treated	Number followed	Percent followed	Number of patients (+)	Percent (+)	Number of (+) patients with no further treatment	Percent (+) no further treatment	Number of patients (±)	Percent (±)	Number of patients (-)	Percent (-)
Immediate .....	781	780	99.9	528	67.7	—	—	61	7.9	187	23.9
30 Days .....	781	710	90.9	353	49.7	—	—	128	18.0	228	32.1
6 Months .....	781	689	88.2	296	42.9	131	19.0	123	17.8	268	38.9
1 Year .....	781	668	85.5	284	42.5	128	19.0	100	14.9	272	40.7
2 Years .....	731	526	71.9	219	41.6	85	16.1	75	14.2	228	43.3
3 Years .....	650	438	67.4	169	38.5	67	15.3	59	13.5	208	47.5
4 Years .....	564	389	68.9	141	36.2	57	14.6	49	12.6	197	50.6
5 Years .....	491	320	65.2	103	32.2	40	12.5	39	12.2	174	54.4
6 Years .....	419	267	63.7	83	31.1	30	11.2	31	11.6	151	56.5
7 Years .....	357	218	61.1	70	32.1	23	10.5	26	11.9	120	55.0
8 Years .....	316	204	64.5	69	33.8	20	9.8	23	11.2	112	54.9
10 Years .....	264	177	66.6	56	31.6	18	10.1	27	15.2	92	51.9
12 Years .....	172	97	56.9	26	26.8	8	8.2	15	15.5	56	57.7
14 Years .....	90	54	60.0	11	20.4	6	11.1	8	14.8	33	61.1

\* All percentages are based on number of patients followed for given period. There were 39 deaths (including 5 due to treatment and 14 suicides) reported during the 14-year period.

had complete reports from this group, it is likely that they would have shown a follow-up course similar to the group we did follow. The effect of these missing data on our statistics would have been to increase the number of patients who were improved or well in the first follow-up years. The number of first relapses and the number receiving subsequent inpatient treatment would also have been considerably larger than reported below.

In order to simplify the data for statistical purposes, the status of each patient was graded in 1 of 3 categories, either +, ±, or -. Under (+) we included patients who at the end of treatment had recovered (free of symptoms, with awareness of having been psychotic), or were much improved (free of symptoms, without awareness of psychosis), or improved (loss of most symptoms, with lessened morbidity, able to adjust outside of hospital); and those during the follow-up period who were out of hospital, either well, improved, working, or at their pre-psychotic adjustment. Under (±) we included patients considered at the end of treatment to have made only slight improvement (such as improvement in behavior in hospital, successful adjustment to convalescent ward but unable to leave hospital because of residual symptoms); and those during the follow-up period who were at home with symptoms, or in borderline psychotic adjustment not re-

quiring hospitalization. Under (-) we included patients who were unimproved at end of treatment, and those during the follow-up period who remained hospitalized, or had recurrences of psychosis requiring hospitalization. Whenever we lacked an actual report of a patient for a given follow-up interval, his status was interpolated, based on data received in later reports.

The immediate result in our series of 781 patients was 67.7% improved or recovered; 7.9% slightly improved; and 23.9% unimproved. Table 1 shows the progress of these persons during the follow-up period. We must emphasize that at least 451 patients, or 58% of all those treated, received some kind of inpatient psychiatric treatment (psychotherapy, ECT, insulin coma therapy, or lobotomy) subsequent to insulin coma therapy at the Pennsylvania Hospital. As noted in Table 1, the number of patients in the improved and recovered group who did not receive subsequent treatment is less than 20% of the total followed at each given follow-up period. The relapse data are presented in Table 2.

Table 1 reveals that the percentage of persons having a favorable follow-up course drops steadily to 32.2% at 5 years after treatment. At 12 years, the percentage falls to 26.8%, and at 14 years only 20.4% of those followed were considered to be well or

TABLE 2

PATIENTS WHO DID NOT RELAPSE; THOSE WHO DID NOT IMPROVE; AND THOSE WHO RELAPSED DURING FOLLOW-UP.

Period of follow-up Years	Total followed	No further Rx. did not relapse		Always (-) no improvement		Always (+) prior to period given period		Number relapsed for first time	Rate of first relapse	Percentages of all relapses reported
		No.	%	No.	%	No.	%			
.12 .....	710	139	19.5	90	12.6	473	66.6	148	31.3	44.3
.5 .....	689	131	19.0	89	12.9	317	46.0	68	21.4	64.6
1 .....	668	128	19.0	86	12.8	246	36.8	44	18.7	77.7
2 .....	526	85	16.1	71	13.5	159	30.2	16*	10.1	82.6
3 .....	438	67	15.3	61	13.9	125	28.5	29	23.2	91.2
4 .....	389	57	14.6	55	14.1	86	22.1	4*	4.6	92.4
5 .....	320	40	12.5	50	15.6	65	20.3	17	26.1	97.5
6 .....	267	30	11.2	45	16.8	38	14.2	—*	—	97.5
7 .....	218	23	10.5	38	17.4	31	14.2	4	12.9	98.7
8 .....	204	20	9.8	36	17.6	24	11.7	—*	—	98.7
10 .....	177	18	10.1	30	16.8	22	12.4	3	13.6	99.6
12 .....	97	8	8.2	18	18.5	9	9.3	—*	—	99.6
14 .....	54	6	11.1	13	24.1	7	12.9	1	14.3	99.9

\* The low relapse rate every second year is an artifact due to manner of assembling relapse data.

improved. Bond (3), in a study of 156 schizophrenic patients admitted to the Pennsylvania Hospital in years prior to the use of the shock therapies, found that 16% of the patients had improved or recovered within 5 years of admission. In a larger study (10), Bond found an 18% rate of improvement or recovery in 5 years, with an additional 8% having improved and relapsed by the fifth year from admission. Using this as a control group, with an improvement or recovery rate of 26%, we note that insulin coma therapy, with a rate of 67.6% immediately improved or recovered patients, appears to be 2½ times as effective in schizophrenia as hospitalization alone without shock therapy. Twelve years after treatment (and in most cases, after various other treatments subsequent to insulin coma therapy), the percentage making satisfactory progress in the treated group is no greater than in the control group.

We did not tabulate our patients' total time in hospital. From a group of insulin treated patients who improved or recovered between 1940 and 1945, Bond found an average hospitalization of 7½ months, with the majority having spent less than 5 months as inpatients. In the control group (1925-34), the average hospitalization time was 9 months for those who improved or recovered without shock therapy. The immediate effect of in-

sulin coma therapy on hospitalization is twofold: it shortens the average hospitalization time, and enables 2½ times as many patients to leave the hospital as would have been expected to leave in the preshock years.

Table 2 shows the posttreatment course of individual patients in certain groups, and may be regarded as a longitudinal sample in contrast with the cross-sectional sample of Table 1. It also shows those who had continuously favorable courses after insulin with no relapses or subsequent treatment; those who remained continuously psychotic throughout the follow-up period regardless of type of subsequent therapy; and those who relapsed following insulin coma therapy. The relapse rate is based on the number of patients who were making satisfactory progress up to the beginning of each given period (Table 2, col. 6 and 7) and therefore were capable of having a relapse.

After 5 years, there were only 40 persons, or 12.5% of those followed, who had remained well without relapse or further treatment. After 5 years, there were 50 patients, or 15.6% of those followed, who had remained sick without improvement regardless of subsequent treatment. It is noteworthy that such a small proportion of those followed failed to reveal any potential for improvement in the first 5 years following insulin coma therapy. That the percentage



continuously ill increases with the length of follow-up reflects the decreasing number of follow-ups received from nonhospitalized patients.

Of the group with a favorable response to treatment, 334 or 63.3% had at least one recurrence of psychosis following insulin coma therapy. Forty-four per cent of all these first relapses occurred within 30 days, and 78% within the first year following treatment (Table 2, col. 10). The rate of first relapse does not appear to follow any definite pattern, but varies between 13% and 30% of those remaining well. The high relapse rate suggests that insulin coma therapy does little to protect the patient from recurrences.

We have correlated the immediate results of treatment with various factors in the patient that may be of prognostic significance, and tested the significance of the data by the chi square method. Our findings closely agree (with the exception of Table 4) with Cohen (4) who reported on 639 patients treated with insulin coma therapy.

The probability of the distribution of numbers in Table 3 occurring by chance is 30%; we conclude therefore that the difference in results between the males and females is not statistically significant.

Table 4 shows a significant association between age of patient and outcome of treatment. The percentages indicate that patients 16 and under have a poorer prognosis than those who are over 16 at time of treatment. The majority of our patients were ill less

than 2 years (Table 5). Had we been treating chronically ill patients, the factor of duration of illness may have cancelled out the significant association between age and recovery. This was apparently the case in Cohen's study.

Table 5 shows a highly significant association between results and duration of illness. The psychotic process is less easily interrupted the longer it continues. In terms of prognosis, 90 days after starting insulin coma therapy 16 out of 20 patients ill less than 6 months can be expected to be able to leave the hospital improved or recovered; after 2 years of illness, only 11 of 20 patients can be expected to benefit significantly.

The results (Table 6) are not significantly associated with the number of coma hours. Since the length of treatment and therefore the number of coma hours is dependent on the clinical response, we believe it is of significance that the optimum results are obtained in that group which received from 30 to 60 hours of coma. In practice, we consider 60 hours of coma a minimum course for those who do not show a favorable response, and we much prefer to give such patients 60 to 100 coma hours before terminating therapy.

There is no statistical association between results and the administering of ECT. As in Table 6, the factor of selection has to be taken into account. The group given ECT had not made good progress on insulin alone

TABLE 3

## IMMEDIATE RESULTS AND SEX OF PATIENT

Sex	+	±	-	Total	% +	% ±	% -
Male .....	212	28	89	329	64.4	8.5	27.1
Female ...	316	33	103	452	69.9	7.3	22.1

Chi Square ( $\chi^2$ ) = 2.607.  
Probability = 30%.

TABLE 4

## IMMEDIATE RESULTS AND AGE OF PATIENT AT TREATMENT

Age	+	(± or -)	Total	% +	% (± or -)
16 or under..	12	14	26	46.1	53.8
17-34 .....	387	185	572	67.6	32.3
35 or over...	125	53	178	70.2	29.8

$\chi^2 = 6.011$ .  
P = between 2 and 5%.

TABLE 5

## IMMEDIATE RESULTS AND DURATION OF ILLNESS

Duration of illness (months)	+	±	-	Total	% +	% ±	% -
Under 6 ...	252	18	40	310	81.3	5.8	12.9
6-12 .....	67	9	26	102	65.7	8.8	25.5
12-24 .....	55	6	31	92	59.8	6.5	33.7
24 + .....	144	28	90	262	54.9	10.7	34.3

$\chi^2 = 50.972$ .  
P = less than 1%.

TABLE 6

## IMMEDIATE RESULTS AND HOURS OF COMA (STAGE 2 AND DEEPER)

Hours of coma	+	±	-	Total	% +	% ±	% -
Under 30 .	77	13	35	125	61.6	10.4	28.0
30-60 ....	240	22	69	331	72.5	6.6	20.8
60 + .....	204	25	86	315	64.7	7.9	27.3

$\chi^2 = 7.393$ .  
P = between 10 and 20%.

TABLE 7

## IMMEDIATE RESULTS AND USE OF ECT COMBINED WITH INSULIN

	+	±	-	Total	% +	% ±	% -
Combined with ECT.....	239	26	86	351	68.1	7.4	24.5
Not combined with ECT.....	289	35	101	425	68.0	8.2	23.7

$\chi^2 = .2110$   
 $P = 90\%$

and might have been expected to make a poorer over-all showing. On this hypothesis it seems that ECT increases the number of positive responses to treatment to the level of the group who made good progress on insulin alone.

Table 8 shows a high degree of statistical correlation between diagnosis and response to treatment. The outcome in the hebephrenic group was much poorer than the expected improvement rate of 67.7%, and the results in the unclassified schizophrenic group were somewhat better than the expected rate. The nonschizophrenics also showed a relatively poor response to treatment.

The likelihood of the results in Table 9 occurring by chance is so great that we conclude that the outcome is not associated significantly with the presence or absence of prior treatment.

Patients receiving a second course of insulin treatment do significantly less well than on the first course (Table 10). We believe

this poorer outcome is associated with the longer duration of illness, since the results of second course correspond closely to those obtained in the group sick 2 years or more. Of the 86 who were (+) on first course, there were 56 (+), 16 (±), and 15 (-) at the end of second course; of the 5 who were (±) on first course, 2 were (+) and 3 (-) on second course; of the 31 who were (-) on first course, 8 were (+), 6 (±), and 17 (-) at the end of second course.

There is a significant association between the weight gained during treatment and the clinical response (Table 11). The more weight gained, the better the outcome. The weight gain probably reflects the weight lost by the patient at the onset of psychosis. We believe that a marked weight loss at onset of psychosis is related to a strong combat against the illness, and is a favorable prognostic sign.

The data in Table 12 are close to statistical

TABLE 8

## IMMEDIATE RESULTS AND DIAGNOSIS

Diagnosis	+	±	-	Total	% +	% ±	% -
Paranoid schizophrenia .....	243	33	76	352	69.0	9.3	21.6
Catatonic schizophrenia .....	51	4	20	75	68.0	5.3	26.6
Hebephrenic schizophrenia .....	36	7	31	74	48.6	9.4	41.9
All other schizophrenia.....	161	13	44	218	73.8	6.0	20.2
Other psychoses .....	24	4	14	42	57.1	9.5	33.3

$\chi^2 = 22.498$   
 $P = \text{less than } 1\%$

TABLE 9

## IMMEDIATE RESULTS AND OTHER TREATMENT PRIOR TO INSULIN THERAPY

	+	±	-	Total	% +	% ±	% -
Previous treatment .....	171	16	66	253	67.6	6.3	26.1
No previous treatment.....	357	45	122	524	68.1	8.6	23.2

$\chi^2 = 1.676$   
 $P = 40\%$

TABLE 10

## THE EFFECT OF A SECOND COURSE OF INSULIN TREATMENT

	+	±	-	Total	% +	% ±	% -
First insulin course .....	86	5	31	122	70.5	4.1	25.4
Second insulin course.....	64	22	36	122	52.4	18.0	28.7

$\chi^2 = 14.304$   
 $P = \text{less than } 1\%$

TABLE 11

IMMEDIATE RESULTS AND WEIGHT GAINED  
DURING TREATMENT (1945-51)

Weight gain (pounds)	+	(± or -)	Total	% +	% (± or -)
0-10 .....	56	35	91	61.5	38.4
10-20 .....	141	44	185	76.2	23.7
20-30 .....	58	21	79	73.4	26.6
30+ .....	43	7	50	86.0	14.0

 $\chi^2 = 11.418$ 

P = less than 1%.

TABLE 12

IMMEDIATE RESULTS AND TOTAL TIME IN  
STAGE IV COMA (1949-51)

Stage IV (hours)	+	(± or -)	Total	% +	% (± or -)
0-1 .....	62	23	85	72.9	27.0
1-2 .....	49	8	57	85.9	14.0
2-3 .....	28	14	42	66.6	33.3
3+ .....	28	5	33	84.8	15.1

 $\chi^2 = 7.049$ 

P = between 5 and 10%.

significance: they suggest a possible relationship between total hours of deep coma and outcome of treatment. When we have more data, we shall report it in further detail, since it has been a matter of some controversy whether deep coma therapy produces better results than light coma therapy.

## DISCUSSION

Our findings reveal a potential for recovery from schizophrenia; 85% of the patients followed showed a capacity for improvement or remission within the first 5 follow-up years, 67% having responded to insulin and 18% to additional inpatient treatment following failure of insulin coma therapy. Only 15% remained continuously psychotic during the first 5 follow-up years (Table 2). It is clear that insulin coma treatment is an effective agent in altering the immediate outlook in schizophrenia.

The long-term picture is not so bright. On the basis of the high relapse rate, it is evident that insulin coma therapy does not produce a permanent resistance to schizophrenia, even though in a few of our cases it appears to have done so. At best, insulin treatment gets the patients back to their prepsychotic level; it is a separate task to help these people maintain this adjustment through sup-

portive psychotherapy, environmental manipulation, or further shock treatments. Even this is not enough. Long-term, intensive psychotherapeutic efforts must be made to bring about a correction of the internal factors that predispose these persons to regress to psychosis. One advantage of insulin coma treatment is that it does not interfere with psychotherapy. The majority of patients are more accessible and emotionally responsive to the therapist during the waking up from coma than at any other time. Whenever the resident or referring psychiatrist is prepared to give the time for intensive psychotherapy, we defer the use of ECT because of its interference with concomitant deep psychotherapy.

Although it has been our primary purpose to review follow-up data, we wish to state briefly our concept of the mode of action of insulin coma therapy. Physiologically, the repeated attacks upon integrated somatic functioning are associated with a marked improvement in autonomic responsivity(5), endocrine function, and body weight. Psychologically, the treatment is almost always experienced by our patients as death followed by return to life(6). The significance of deep coma may be that it produces the feeling of having died; light coma rarely does. The therapy imposes a situation of dependency and helplessness upon the patient, in which the need for feeding and being mothered becomes a matter of life or death. We believe the repeated gratification of these needs does something to lessen the need for regression to psychosis. Also of important psychologic value in the recovery process are the gain in weight, and the group interrelationships that develop among the patients, and between patients and staff. Translated into the practical terms of our daily insulin unit practice, we strive to produce deep coma in the patient, and a warm, mothering attitude in the nurse.

## SUMMARY

1. Insulin coma therapy at the Pennsylvania Hospital produced an immediate improvement or remission in 67.7% of 780 patients treated between 1936 and 1951.

2. At least 334 patients, or 63.3% of all patients who originally improved, had a relapse; 44% of all these relapses occurred within 30 days, and 78% within 1 year of treatment. A second insulin course brought about an improvement or remission in 52% of 122 patients who had relapsed.

3. Factors associated with the most favorable prognosis include: age over 16, psychosis of less than 6 months' duration, with a clinical picture of paranoid, catatonic, or undifferentiated schizophrenia; and if during treatment the patient receives at least 30 to 60 coma hours and gains more than 30 pounds in weight.

4. We conclude that insulin coma therapy is effective in restoring the schizophrenic patient to his prepsychotic adjustment. This restoration to health is not accompanied by a permanent correction of the factors that

predispose the patient to regress to schizophrenia.

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## THE EEG CHANGES IN UNILATERAL AND BILATERAL FRONTAL LOBOTOMY<sup>1</sup>

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Though the early postoperative changes in the electroencephalogram following surgical lobotomy have been well described in the literature(1, 2, 3), little study appears to have been made concerning the duration or reversibility of these changes. In addition, the possibility that the postoperative tracings may in some way be correlated with the degree of clinical improvement also appears in need of investigation. With these considerations in mind, an attempt has been made in this study to construct a composite picture of the electroencephalographic changes occurring as a result of unilateral and bilateral surgical lobotomy.

### CASE MATERIAL

Tracings from 150 patients were utilized in this review. The majority of cases were psychotic. They had been institutionalized for relatively long periods and had not responded to other forms of somatic treatment. Of these cases, 125 were diagnosed schizophrenia (43 paranoid, 30 catatonic, 18 hebephrenic, and 34 of mixed type). In addition there were 14 cases of involuntional melancholia, 5 with manic-depressive psychosis, 4 with obsessive compulsive psychoneurosis, and 2 cases of psychosis with epilepsy. The duration of illness was as follows: 11 patients had been ill from 1 to 2 years, 52 from 3 to 5 years, 44 from 6 to 10 years, 29 from 11 to 15 years, 7 from 16 to 20 years, and 7 from 21 to 35 years. One hundred forty-three were females and 7 were males. This disproportion resulted from the character and physical limitations of the wards in that only a limited number of male patients could

be accommodated. Age distribution was as follows: 5 patients were in the second decade of life, 32 in the third, 53 in the fourth, 34 in the fifth, 21 in the sixth, and 3 in the seventh.

### OPERATIVE PROCEDURES

One hundred seventy-one surgical procedures were performed on the 150 patients; 117 were bilateral, Poppen type operations in which the white matter was sectioned through burr holes in the plane of the coronal suture and the sphenoidal ridge; 54 operations were unilateral, 27 on the left and 27 on the right. Many of the patients had 2 separate unilateral operations, the last being on the opposite side and separated by a period of from 4 to 7 months; 9 patients had left-sided operations only and 5 had right-sided operations only. Two other cases are included, 1, a cingulate topectomy on the left, and the other a severance of the fibers from the cingulate areas bilaterally. All cases were operated upon between 1949 and 1952. They were selected for this study solely on the basis that an adequate pre- and post-operative EEG tracing had been secured. This would then exclude those cases who died shortly after surgery; hence no adequate statement can be made regarding operative mortality in this series.

### EEG TECHNIQUE

The 458 records in this study were all obtained using a 6- or 8-channel Grass machine. Needle electrodes with both ear-to-scalp and scalp-to-scalp technique were used routinely. All major areas of the head were covered, including the frontal, temporal, parietal, and occipital regions. Tracings in all cases were secured preoperatively and 9 days postoperatively, with continued serial tracings in many patients as long as they could be followed, in some cases for a 1- to 3-year period. In interpretation, particular attention was given to focal activity, the

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amount and frequency of the slowing of a nonparoxysmal type, and areas exhibiting asymmetry and asynchrony. All records which demonstrated the slow activity were further divided into 3 groups as follows: tracings that revealed only mild degrees of slowing, usually in the 4-7 cps frequency range, tracings showing moderate degrees of slowing with 2-4 cps activity, and tracings containing a great deal of 1-4 cps activity of wide distribution. In addition, records showing paroxysmal cerebral dysrhythmia were classified into 3 groups: mild, moderate, and severe, based on the quantitative amount of paroxysmal activity, as revealed by inspection.

#### FINDINGS IN THE PREOPERATIVE RECORDS

Seventy tracings, of the 150, taken preoperatively from each patient were considered to be normal, and 18 of the records exhibited low voltage fast activity; thus, making a total of 88 records considered within normal limits and 62 abnormal tracings. Forty-four records of the abnormal group contained varying amounts of paroxysmal dysrhythmia; 10 of the records contained both paroxysmal and nonparoxysmal slow activity, and 5 were considered to contain mild degrees of slow activity. Three of the 150 records displayed focal abnormality, without any clinical neurological findings. Twenty-nine of the 62 abnormal records were interpreted as containing abnormalities most probably related to electroconvulsive therapy administered within the preceding 3 weeks of the preoperative tracing. For the most part, this consisted of slow nonparoxysmal activity of a nonfocal nature.

#### RESULTS AFTER BILATERAL LOBOTOMY

One hundred of the 117 records taken 9 days after a bilateral operation contained the greatest degree of slowing in the frontal areas. However, in only 21 of these records the slow activity was found to be limited to the frontal leads, and a rather wide variation was encountered, in that slow activity appeared in many nonfrontal positions. In 33 of the 117 records, the slowing, as characterized by 1-4 cps waves, appeared in descending prominence both in frequency and

amplitude in the frontal, temporal, and parietal areas, and to a lesser extent in the temporal leads. Nineteen of the records contained fronto-temporal slowing, and 5 records demonstrated fronto-parietal slowing. Other variations were found in 8 records with the greatest degree of slowing in the temporal rather than the frontal leads, and one record with the greatest degree of slowing in the parietal lead. Also, in those with bilateral operations, 5 of the records contained unilateral slow foci in the frontal areas, perhaps indicating the difficulties encountered in the placement of symmetrical surgical lesions. Three records taken during this postoperative period were within normal limits. Twenty of the 117 records contained prominent asymmetry and asynchrony in the temporal areas.

#### RESULTS AFTER UNILATERAL LOBOTOMY

In the postoperative tracings taken 9 days after the initial unilateral procedure, focal slowing on the side of surgery was apparent in 26 of the 34 cases. In 4 cases the slowing was equal on the two sides, and in 4 cases the slow foci appeared on the side opposite the surgical procedure. Following a second unilateral operation, the EEG picture becomes more complex, but in general it appeared that the record from the most recently operated side was characterized by higher potential and greater slowing extending over the frontal, temporal, and parietal areas. After a period of 2 to 3 months, the records were indistinguishable from those obtained from patients with bilateral operations.

#### DURATION OF EEG ABNORMALITY

*Bilateral Lobotomy.*—Serial tracings conducted on bilateral lobotomized patients in the postoperative period demonstrated a gradual reduction in potential of the slow activity and an increase in its frequency. In a group of 42 patients followed for one year, only 5 had normal records. The remaining 37 records demonstrated predominantly mild to moderate degrees of slow activity limited to the frontal areas. In another group which was followed for 2 years, 2 of 29 cases had normal records, with the records of the remaining 27 cases again showing slow frontal

activity. Another group followed for 3 years had abnormal records in 17 out of 19 cases, again demonstrating slow activity of mild to moderate degree in the frontal area. The remaining 2 cases had normal records.

**Unilateral Lobotomy.**—In the 14 patients undergoing a single unilateral operation, 3 had tracings taken more than one year after surgery. One of these was normal, and the other 2 contained moderate degrees of slow activity, limited to the frontal area of the operated side. In the group of patients having 2 separate unilateral operations performed, 8 had tracings taken from 2 to 3 years after their last surgery. Only one record in this group was normal, the other 7 showed mild to moderate slowing in the frontal leads of about equal prominence on the two sides.

**Postoperative Seizures.**—Aside from the immediate postoperative period of 10 days, 17 of the 150 cases had clinical seizures following surgery, while only 2 patients had preoperative seizures. The convulsions in 15 cases may then be considered as being related to surgery, giving an incidence of 10%. Though seizures occurred in the group undergoing single unilateral lobotomies, the group is not sufficiently large (14) to make a valid comparison with the bilateral group (117) in regard to the incidence of postoperative seizures. The preoperative records in the group of patients eventually developing convulsions demonstrated paroxysmal activity in only 2 of the 17 cases. However, the postoperative tracings taken 9 days to 6 months after surgery, in 10 of the 17 patients with convulsions, exhibited definite paroxysmal activity.

#### RELATIONSHIP BETWEEN THE EEG AND CLINICAL IMPROVEMENT

One hundred and ten patients in this series have been followed clinically for 6 months or longer. These included patients with both unilateral and bilateral operations. According to their clinical status, these patients were divided into 4 groups as follows: those showing no improvement after surgery, those showing slight improvement, and those showing moderate or marked improvement. The criteria used for the selection of these

TABLE 1  
EXTENT OF CLINICAL IMPROVEMENT

No. of patients	None		Slight		Moderate		Marked	
	No.	%	No.	%	No.	%	No.	%
Normal records ..	0	0	2	10	1	2	0	0
Slightly slowed records .....	1	11	4	20	3	6	2	8
Moderately slowed records .....	5	55	9	45	41	83	22	81
Markedly slowed records .....	3	33	5	25	4	8	3	11

groups have been described in a previous paper(7). The postoperative EEG's taken 9 days after surgery on these cases were then classified into 3 groups depending upon the degree of slowing, whether slight, moderate, or marked. Table 1 and Fig. 1 indicate that the percentage of records containing degrees of slowing is different in the 4 clinical groups. Slowing of a moderate degree is most prominent in those patients who had moderate to marked clinical improvement, in contrast to the high percentage of markedly slowed records in those who exhibited only slight or no symptomatic improvement. The percentage of mildly slowed records is also higher in this latter group.

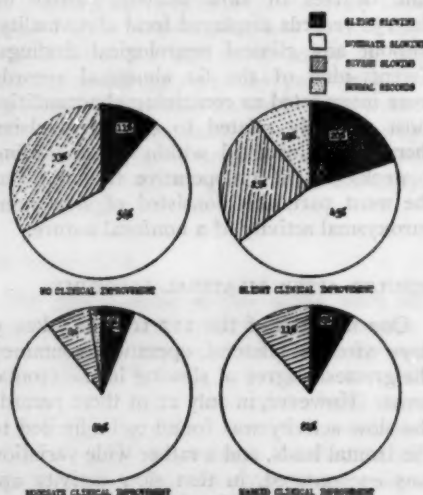


FIG. 1.—Graph demonstrating the percentage of records showing different degrees of slowing in relation to the patient's postoperative clinical status.

## DISCUSSION

A composite picture of the changes occurring after bilateral lobotomy in the EEG would appear to be as follows: In the first 2 weeks, the records are characterized by high potential, random, 1-4 cps activity most prominent in the frontal leads. This study is in agreement with the work of Davis, Cohn, Levin, and Freeman and needs no further elaboration. However, during this period, slowing, usually of less prominence, also appears in the temporal tracings, and to a still lesser degree, in the parietal leads. Asymmetry and asynchrony may be quite prominent in the temporal tracings. By 6 weeks, the records have become more focal in the frontal leads, and the slowing in the temporal and parietal areas has disappeared.

In this study, in all but 2 of 19 cases followed for a period of 3 years in the bilateral group, slowing of a mild to moderate degree persisted in the frontal leads. In the group receiving 2 separate unilateral operations and followed for a 2- to 3-year period, 7 out of 8 records demonstrated persistent slowing of a mild to moderate degree, again limited to the frontal leads. This finding is somewhat at variance with the duration of abnormality discussed by Cohn (1945), who states that the return of the electroencephalographic pattern to a relative normal rhythm usually requires 1 to 3 months. The one case studied over a 2-to-4-year period described by Freeman (1950) had an essentially normal record at that time. Levin (1950) states that baseline frontal oscillations often continued for months or years after the operation, but does not discuss the percentage of cases in which the persistent slowing occurs. In our group, the percentage would appear to be 89.5% (17 of 19 cases) showing a persistent change after 3 years in the bilateral group, 87.5% (7 of 8 cases) of the unilateral group receiving 2 operations also demonstrated persistent change over this period. The group undergoing single unilateral operations was not sufficiently large to draw conclusions on the basis of percentage; however, the same trend appeared evident.

From this study it would also appear that in spite of the attempt to perform a specifically localized surgical procedure, surprising variations occurred in the degree and posi-

tion of slow wave activity, though it was usually transitory in the nonfrontal locations. A possible explanation for the slowing in the temporal areas in particular is the presence of cerebral edema and hippocampal herniation resulting from operative manipulation (6).

In this study, there is no definite suggestion that an abnormal preoperative tracing, as characterized by paroxysmal dysrhythmia, is correlated with the appearance of postoperative convulsions.

However, a correlation between the degree and extent of slow wave activity found in the early postoperative period and improvement of the patients' mental condition as observed clinically is suggested. This finding is not in agreement with the work of Stevens and Mosovich (8), who found no correlation between EEG pattern and postlobotomy improvement. It was noted in our study that patients showing moderate slowing in the EEG showed greater clinical improvement than those with either slight or severe EEG slowing. Since the degree and extent of slowing in the EEG seems to be related quantitatively to the extensiveness of the surgical lesion in the frontal lobotomy operation, it would appear from this study that an optimal extent of the surgical lesion, as reflected by the amount of slow activity in the EEG, is an important factor in bringing about a clinical improvement. Thus it may be reasoned that a mild slowing in the EEG is an indication that the surgical lesion is not sufficiently extensive to bring about an appreciable change clinically; whereas, with severe slowing, the surgical lesion may be considered as too extensive for clinical improvement.

The question arises as to the correlation of the EEG changes in known surgical lesions, such as the type described in this paper, with the type of changes arising from incidental lesions resulting from trauma. This question was not investigated at this time, but rather the study was directed toward the correlation between EEG changes and clinical alterations brought about by a specific type of cerebral lesion.

## SUMMARY

1. In cases of unilateral and bilateral frontal lobotomy, temporal slow wave activ-

ity was found to be a prominent feature, though more transient than the dominant frontal slowing. This nonfrontal slowing may be interpreted as the result of cerebral edema and hippocampal herniation from operative manipulation.

2. There is an apparent correlation between the degree of slow activity in the post-operative EEG and clinical improvement, in that slight or severe degrees of slowing was found to be more conspicuous in those cases that exhibited little clinical improvement. In contrast, moderate slowing was associated with moderate to marked clinical improvement.

3. The frontal slow wave activity, though decreasing in prominence, persists in the majority of cases for at least 3 years and possibly longer.

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## ON THE PSYCHODIAGNOSTIC VALUE OF HANDWRITING ANALYSIS

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### THE PSYCHOLOGICAL RATIONALE OF HANDWRITING ANALYSIS

The value of handwriting analysis as a psychodiagnostic tool is based not only on the theoretical assumption that an individual is essentially self-consistent and that such self-consistency is reflected in one way or the other by *all* his behavior and actions, it is more specifically based on two facts which are almost self-evident, but which were investigated and proven correct experimentally: (1) Handwriting is at the same time a product and a permanent record of a person's highly individualized motions. (2) There is an intricate and interpretable relationship between an individual's motions and his emotions.

The existence of a correlation between motions and emotions is the premise for much of what we call "clinical observation," whether we diagnose a patient as euphoric or depressed, manic or catatonic, is certainly influenced by the motions we observe.

The question of the individuality of handwriting was experimentally investigated and statistically confirmed, but for a long time prior to these statistical studies, its existence was common knowledge and one of those facts which just work. We see it daily when we look at the mail we receive, when we present a check to the teller at the bank, who knows exactly that there is only one person who could have produced the motions that resulted in the signature of his customer. The smooth working of our whole economic system, as well as our civilian and criminal legal procedure, depends largely on the correctness of the assumption of the strict individuality of handwriting.

The psychological reason for this generally accepted and utilized individuality of handwriting is rooted in the fact that *handwriting is self-recorded behavior in a structured situation.*

The structure within which the individual behaves while writing is provided by the requirement for a minimum legibility within the framework of the national alphabet. It is furthermore influenced by the style which the specific cultural era, in line with its psychological characteristics, employs in the use of this alphabet. Why certain cultures develop their specific alphabet, what the formations thus produced express, as well as the investigation of historical changes of alphabets, makes a fascinating study, but it is mentioned here only to stress the value of expressive movement as recorded in handwriting.

Within these limits, provided by the requirement for a minimum legibility and by the national alphabet, influenced by its contemporary styling, it is left to the writer to form and connect his strokes and to distribute the available writing space in the course of his actions.

Next to language, movement of our body or its parts—gestures—is our most common means of expression. In the development of the human race expression by movement preceded the use of organized language, and the same is true of the development of the individual. In times of emotional shock one might lose his ability to express himself by language, his "tongue freezes" temporarily, and he regresses to the earlier, as it seems, more deeply rooted, state of expression by gesture. It is not a premise for the validity of the expressive and projective values treated herewith, but it is quite possible that, because our earliest reactions were freely expressed by movements, the expression by movement continues to be more free, less inhibited than verbalization.

Even after the use of language has been well established, the individual—as did mankind in its development—continues the use of movements as means of expression. Gestures may be conscious, intended to support the word, or they may be unconsciously produced, sometimes even against our will, be-

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traying the untruthfulness of the spoken word.

When man learned to preserve the meaning of language, first by using drawings as his notes, later by using the symbols of letters, language had gained its decisive preponderance over expression by gesture. The gesture remained spoken into the wind—and gone with the wind. Now, in the decades of widening use of projective and expressive techniques, it is well to remember that handwriting does not only convey the content of the written word; it is, at the same time, a product and a permanent record of our movements. It is, to use Allport's and Vernon's expression, "crystallized gesture."

That our mental processes influence our movements was demonstrated first, as it seems, by M. E. Chevreul in his famous experiments with a pendulum. These simple experiments can be easily repeated by anyone. A subject is handed a ring which is suspended from a hair and he is requested to hold the other end of the hair motionless between his fingers. He is then asked to think, without looking at the ring, that it is moving, prescribing a circular movement. The ring will, as an effect of the subject's thought-produced motions, start moving in a rotary fashion: clockwise when the subject thinks of that direction, counterclockwise when he imagines the opposite direction. Chevreul reported that the perception of such effect increases the movement. This principle should increase the expressive value of handwriting, as we observe our movements and their record while we write. Later studies went into numerous other details in investigating the connection between mental processes and movements. The most important experimental investigations are the studies by Allport and Vernon, Lewinson, and Zubin, Saudek, and the various publications by Wolff.

The word "handwriting" is thus a misnomer. Our hand does not write; it is a tool used for the double purpose of consciously communicating thought content and unconsciously expressing personality. Instead of this tool, other tools, other parts of the body, can be used and are used, and because they are expressive of the same personality they are bound to produce the same graphic rec-

ord. Those patients who become unable to further use the hand with which once they wrote or who lose the use of both hands will, after a relatively short training, produce essentially the same letter formations as before, now with their other hand or by holding the pencil between their toes, or in their mouth.

#### WITH RIGHT HAND

*I have only say that  
I have been more than  
one hundred times  
engaged in Battle  
Horatio Nelson  
May the God of battles  
crown my endeavors  
with success  
Nelson for ever*

#### LEFT HAND TEN YEARS LATER

FIG. 1.—Specimen of Lord Nelson's handwriting showing similarity between left and right hand. (Original size.)

The writings in Fig. 1 originate from the same person, and exactly because on first glance they do not appear similar they were chosen to demonstrate the principle of essential consistency of handwriting. Both were produced by Horatio Nelson. The upper writing is a product of his right hand; the lower was written 10 years later with his left, after Nelson had lost his right hand. Saudek, in a quantitative study of numerous handwritings, established that the element most consistent and, therefore, most difficult to falsify is the exact individual relation between the upper length and the lower length of letters. In fact, this relation between the length extending above and the length extending below the line does not change at all. A quantitative consistency in the organization of the available field of action—spacing of letters and words—too, was found. However, these two Nelson writings show, in addition, identities that can be seen without microscopic measurements, by pure inspection. Comparison is made easy as the 2 specimens contain one almost equal word: "battle," with 6 letters in the right hand writing,

and "battles," with 7 in the specimen produced by the left hand. In both specimens:

1. The "B" is written like a "13."
2. The "1" in the "13" is larger than the subsequent "3."
3. The writer interrupted completely after the "B," while the rest of the writing is completed without further interruption.
4. The distance between the "a" and the following "t" is longer than any of the following interword distances.
5. The first "t" is larger than the second.
6. The second "t" has no "t" bar of its own.
7. The "t" bar of the first "t" forms a tangent, which exactly touches the top of the second "t."

Numerous other absolutely equal movements were produced, but those described should suffice to impress the fact of individuality of movement pattern, the more so as 10 years elapsed between the writing of the 2 specimens, years filled with adventure and battle, and the 2 specimens are produced with different hands.

Such identities of movement pattern, produced by different groups of muscles and separated by many years prove more conclusively than anything else that deeply rooted central dispositions in personality play an important part in the formation of handwriting.

In the past decade the theory and development of situational tests have received considerable attention. Handwriting analysis is not only, but it is also, a situation test. The test material is most easy to procure and the test itself is, quite paradoxically, self-administered, without the testee's knowledge. Every single letter we write contains numerous individual situational problem solutions. Whenever we have to draw a line we are faced with the problem of connecting two imagined points. To mention just some of the interpretable behavior: whether we solve this problem by drawing a straight and firm line, clearly shooting toward the aim; or whether we connect these points with a hesitating and wavy stroke; whether we seek the shortest solution; or, for the purpose of appearance, lose ourselves in detours and delight in intricate embellishments and flourishes; whether we put letters firmly and largely into the available space; or produce small letters with weakish pressure, is a recorded outcome of our behavior in this continually fluid and changing situation.

Thus, handwriting analysis as a situation test has the advantage over other such tests in that the behavior is not only self-recorded but, in addition, is a test of behavior in *numerous* situations which, like the ribbon of a film, flow by the writer's eyes as his pen moves across the paper. There are numerous ways of forming and shaping a stroke, many ways of connecting strokes with each other. Also, the subject might write with large letters or with small ones, with rising lines and soon his letters might become small and the lines show a declining trend. If one combines all these possibilities of individual behavior within the framework of the school copy, one arrives, because of the combination of numerous possibilities, at astronomical figures.

This individualization and deviation from the stereotype school copy starts with the very first writing lesson in grade school and it never ends. In this very first lesson we can see the teacher instructing all the children by the same method; they all use the same writing material. Yet, one child will use up the whole writing space with a few bold strokes, while another, expressing his personality, will hide his weakish and wavy lines in one corner of the space. One writes with meticulous care, thus displaying compulsive elements; while the other one will not care for the details, solve the problems carelessly with untidy result. Increased facility in writing and the differentiation of personality increase the potential for individual expression. Therefore the more intelligent, mature, and original the writer, the less strict will be the adherence to the school copy.

#### SOME PSYCHODIAGNOSTIC ASPECTS OF HANDWRITING

##### THE INDIVIDUAL AND HIS LIFESPAN: PRESSURE AND COUNTERPRESSURE

Pressure in handwriting is, psychologically seen, the graphic record of our reaction to the resistance created by the friction between paper and pen. In his effort to overcome this resistance, the individual's pressure may be applied in a consistent manner, or it may increase towards the end of strokes or words. It may diminish or fluctuate, resulting in irregular pressure. The endeavor to go ahead may block when facing certain tasks. The

resulting blocked pressure may show when the writer is confronted with the need for a complete turn of direction, or for a dive into a deep form valley, or when the pen climbs to a peak.

Graphic pressure in drawing and painting, as well as in handwriting, is often interpreted as a sign of "energy." Psychoanalytical handwriting analysts in particular describe pressure as the graphic expression of libido. In the widest sense, pressure is indicative of application of "energy" in some way, but it cannot be said that it is an expression of energy in the sense of strength. Whether it indicates organized application of strength, or is a product of the frustration and tenseness of a neurotic, has to be understood by looking at pressure as one kind of solution in the test of writing. Particularly Klages and Pulver probed this aspect of writing. The way a person deals with the problem of the obstacle of friction is considered one indication of his method of dealing with obstacles. He may react by increasing his pressure, by using increased force to move his pen, and such increase might reach the point of brutal damage to the paper. On the other hand, he might try to avoid the difficulty, by reducing the pressure, to experience less friction and to move the pen with more ease. The extreme case of such test reaction, or test solution, is the pen which easily "dances" across the paper, producing lines so thin and lacking in distinctness that they are almost invisible. Such a writer in this situational test has exhibited—and himself graphically recorded—full subordination of his own desires for expression to the requirements of the surroundings.

Yet, it would be erroneous to assume that the writer who treated the paper so forcefully—and brutally—is a forceful personality. Such performance might very well be the result of confused and disorganized behavior in the face of a problem and, therefore, actually indicate weakness instead of strength. On the other hand, weak pressure might be the record of the expressive movements of a subject who will not waste his energy in blind attacks but adapts himself to the opportunities, marshals his strength, and employs it in organized attack, where the aim

cannot be reached without it. The general rule of expressive and projective techniques, that one response derives its meaning only in connection with the general constellation in which it is found, is particularly true in the analysis of handwriting because of its extreme complexity of possibilities of expression. Especially we cannot assess energy from the pressure in handwriting without studying the subject's ability to employ his strength in an organized way.

#### THE SELF CONCEPT IN HANDWRITING

It is an accepted assumption that, for example, in children's drawings the size of the figure the child draws of himself, its relative position to the surrounding figures, and other characteristics of the drawing are quite indicative of his self-concept. The drawing of the letter "I" invites the same identification procedure. Subjects who view themselves as weak and insecure unconsciously reveal this when they write. They do not dare to put down a forceful, heavy-pressured, and rhythmically easily produced "I." Their "I" will lose in relative size when compared with the surrounding letters; it will not reach as proudly up as the "I," the self-portrait, of a subject who views himself with self-confidence and invites inspection—and maybe even admiration—of himself. An "I," symbolized self-portrait of a subject of low self-esteem, might lose not only in height but also in pressure or seek the protecting nearness of neighboring letters.

Signature, however, has an additional psychological meaning, exceeding the one of the letter "I." While writing the context a subject feels relatively unobserved (if he does not write knowingly for analysis purposes). When, however, he comes to draw his signature, he feels that he is putting himself into the limelight. The signature is on public display. In the document he will write more or less in his natural way, while in the signature he will feel inclined to present himself in the way he wants to appear. Differences between writing characteristics in the signature (larger or smaller size, showy embellishments versus functionally simple formations, firm versus wavering strokes, etc.) are, therefore, particularly revealing.

Benito Mussolini  
 Mussolini  
 Mussolini

FIG. 2.—Developmental change in Mussolini's signature. (Original size.)

Fig. 2 shows developmental changes in Mussolini's signature. The top signature was written when he was a fairly unknown journalist. The middle one, just after he came to power. (Notice, besides the increased size, the omission of the first name. Dictators and gods have one name only. Napoleon, Hitler, and most persons who gained dictatorial power dropped their first names after they achieved such status.) The last signature was written after Mussolini, in an attempt to assure himself a large slice of this world's surface, joined the Germans in the war against the Allies.

Even more specific unconscious projections of the self-concept are found in handwritings. When a child will draw himself as a roaring tiger or as a frightened rabbit, most psychologists will not hesitate to adjudge considerable projective meaning to it, particularly if the subject continuously produces the same identification. In our daily life we are meeting with hundreds of such graphic identifications; yet, in spite of their projective value, we do neglect their interpretative possibilities.

Fig. 3 shows the signature of the Dutch naval hero, Marten H. Tromp, who in the days of Oliver Cromwell destroyed both the Spanish and the Portuguese fleets and came close to dealing the same fate to the British navy. He was known "to live on sea only." His flagship, of whose performance he was exceedingly proud, was his home. The identification is quite obvious.

But one does not have to go back to his-



M. H. Tromp

FIG. 3.—Signature of Dutch naval hero, Marten H. Tromp, showing identification with his flagship. (Original size.)

torical personalities to see extensions of the ego projected clearly into handwriting. Our magazines and newspapers often publish, together with pictures, the handwriting of the photographed person, and by pure inspection one will find confirmed the same possibility of interpretation.

When Walt Disney's pen moves across the paper it performs the same angular and discrete movements which characterize the motions of his animated cartoons (Fig. 4). In addition, the "i" dot over his second name is not a dot at all; it is more similar to Disney's famous creation, Mickey Mouse. Equally, the initial letter of his second name has less similarity with this letter than with the rear part of Disney's famous dog, Pluto.

Equally, Mr. I. J. Fox, nationally known American furrier, appears to be quite strongly extending his self to his occupation.

Walt Disney  
 I. J. Fox

FIG. 4.—Signatures portraying identification with occupation. (Original size.)



He is quite "Fox conscious," as shown by the fox tail in his signature. Yet, he was surprised when made aware of it by the writer.

Many such unconscious projections in handwriting are quite amusing, exactly as amusing—but not less revealing—as it is to observe a child again and again draw a picture of his family and, for example, putting a heart into his father's but not into his mother's chest.

#### A DIFFERENTIAL DIAGNOSTIC INDICATION IN PSYCHOPATHOLOGY

The understanding of the dynamics of a patient can, as outlined above, be greatly facilitated by the analysis of his unconscious graphic expression and projection. Handwriting analysis is often called a projective technique; however, it by far exceeds the possibilities of both a situation test and a projective technique. It has, in addition, an aspect which it shares only with such objective devices as the electroencephalogram or electrocardiogram. Handwriting is the result of a brain wave; it is the transformation of a symbol into motion recorded on paper.

Physiologically seen, writing is produced by the binding and releasing tendencies of the muscles. The relation between binding and release constitutes the rhythm of handwriting. All the other specific handwriting characteristics, like size, pressure, slant, etc., are interwoven into this rhythm. It interrelates them all. Disturbances of this rhythm, which is essentially the balance between muscle release and tension, are recorded on writing paper, much like the electroencephalogram or electrocardiogram. The study of minute disturbances of rhythm has yet hardly started, but somewhat grosser irregularities are by now interpretable, with a good systematic presentation of the problem provided by Sonneman.

If we combine the study of handwriting as a behavior pattern with its study as a rhythmic change of tension and release, we can take the catatonic schizophrenic as an extreme case of binding, the actually manic patient as an extreme case of release.

The affective disorders, mania, hypomania, and the various kinds of depression, to the degree to which they can be at all differentiated from schizophrenic disorders, show

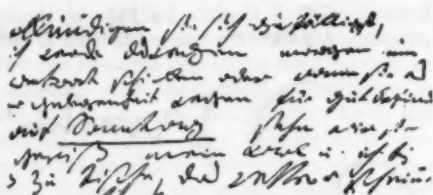


FIG. 5.—Handwriting of Beethoven indicating manic-depressive characteristics. (Approximately one-half original size.)

whatever difference exists in the quality of rhythmic disturbance. In line with the different dynamics, the writing of the manic does not record—as that of the schizophrenic does—disintegration. It does not show the turmoil of disorganizing conflict between the impulses of flexion and release. The manic's writing is characterized by the exaggeration of its dimensions and its directions, but not by a disturbance of rhythm. Rhythm, to the contrary, appears easy flowing and even accentuated. The manic's writing is often aesthetically pleasing.

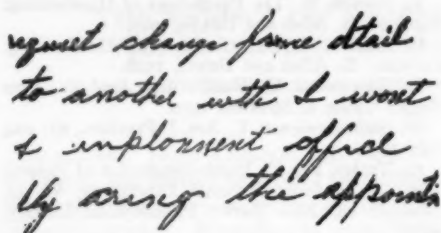
As we know, genius is sometimes manic. His writing (Fig. 5) confirms that Beethoven, the man with the wild-flowing hair, who in the last century was so often seen running through the streets of Vienna laughing and singing, whistling and wildly gesticulating, bumping into people, that this genius was what professional biographers diagnosed him—a manic-depressive. Equally, the motions recorded in Charles Dickens' handwriting (Fig. 6) support the biographers who diagnosed him as a manic-depressive.

In the manic-depressive psychosis, depressive type, we observe the increase of binding, with falling lines and toppling-over of letter formations, without disturbance of rhythm. The script flows slowly but rhythmically.



FIG. 6.—Charles Dickens' signature showing manic-depressive traits. (Original size.)





request change from detail  
to another with I want  
a employment office  
by using the appointment

FIG. 7.—Handwriting of a depressed schizophrenic.  
(Two-thirds of original size.)

The depressed schizophrenic (Fig. 7) will not show just an exaggeration of binding tendencies, but also conflict between binding and releasing, with the binding predominant in a dissonantly deteriorated rhythm.

Depending on the form of schizophrenia, the schizophrenic's writing might also show—but not necessarily—exaggerated dimensions. Altogether, congruent with the dynamics involved, the schizophrenic's writing has an essentially disintegrating centrifugal quality, the cyclical one is essentially centripetal. The schizophrenic's handwriting, because of the deteriorated rhythm, gives it the distinct feeling of being brittle, the cyclical of being more pliable.

### SUMMARY

The basic psychological rationale of handwriting analysis is rooted in the essential self-consistency of human behavior and more specifically in the fact that handwriting is a product and, at the same time, a record of highly individualized motions. The rationale is furthermore based on the existence of a correlation between our motions and emotions. The individual's intrapersonal consistency in hand writing is discussed and 3 methodological approaches to the analysis of handwriting are, proceeding at different levels, distinguished, with their findings merging into one final analysis.

1. Handwriting analysis as a situation test: the individual's behavior and its dynamics in the structured situation of handwriting are analyzed, the structure being provided by the contemporary national alphabet and the requirement for a minimum legibility. As an example of the numerous possibilities of interpretation, the dynamic signifi-

cance of the application and distribution of pressure is discussed.

2. The graphic character of the activity provides numerous possibilities for symbolic expression. As an example of this, the symbolic expression of the self-concept is discussed and demonstrated by samples.

3. Physiologically, handwriting is the result of muscular contraction and release. Disturbances in the quantity, direction, and balance between contraction and release, and the resulting rigidity and deterioration of rhythm in psychopathology are discussed and a diagnostic indication between cyclical and schizophrenic disorders is pointed out.

It should be stressed that many of the potentialities of handwriting analysis are still in their early development, though some of them have been more fully investigated than can be discussed within the limits of this paper.

Two specific results of the uniquely easy availability of the test material should at least be mentioned. One is the preventive value. Often a casual inspection of a handwriting might cause justified suspicion and result in further psychiatric and psychological investigation.

The other, and even more singular, potential was purposely hinted at by introducing the writing of historical personalities. Those who lived long before other psychological tests could be administered left behind them in their handwriting a record which provides the possibility of a test-based psychological evaluation. The clinician can, therefore, be helped to a more objective understanding of such figures in a patient's life who have long been deceased. Handwriting analysis opens new vistas to the understanding of men and women in history.

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## THE EFFECT OF ADRENOCROME AND NIACIN ON THE ELECTROENCEPHALOGRAM OF EPILEPTICS<sup>1</sup>

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In a recent paper Osmond and Smythies (1) again emphasized that there is a clinical similarity between schizophrenia and the mescaline experience. Mescaline is structurally similar to adrenalin. They suggested that it would be worthwhile to search for a hypothetical M substance somehow related to both. One year later Hoffer, Osmond, and Smythies (2) reported that 4 hallucinogens had indole nuclei and that adrenochrome, a natural derivative of adrenalin with an indole nucleus, had properties that one would expect of a schizophrenic toxin: (1) Psychologically adrenochrome in normal volunteers induced changes analogous to those induced by lysergic acid. (2) Biochemically, adrenochrome markedly inhibited *in vitro* brain respiration. It causes a marked inhibition of respiration by blocking the Krebs cycle and the hydrogen carrier diphosphopyridine nucleotide system (3). (3) Neurophysiologically, adrenochrome induced EEG abnormalities.

In this paper we report the neurophysiological studies to which Hoffer, Osmond, and Smythies (2) referred.

As it is a well-known fact that the brain represents an open energy system which produces energy mainly by carbohydrate oxidation, it was felt that it would be desirable to investigate the neurophysiological aspects of adrenochrome to determine the possible changes in EEG caused by this drug. As the epileptic brain is susceptible to any change in the external environment and internal milieu (4, 5), we carried out our investigations on epileptics.

Nicotinic acid recently has become an important drug for treating psychiatric conditions. Washburne (6) used it in cases of depression; Hoffer (7) found it useful in certain types of schizophrenia; Gould (8) reported favorable results in alcohol and barbiturate intoxications. Recently workers have reported (9) beneficial effects in preventing neuritis, an epileptic discharge or side effect of isoniazide treatment. Taking into consideration these facts, we felt that it would be interesting to include the effect of nicotinic acid on the EEG after adrenochrome.

### MATERIAL AND METHOD

EEG investigations were carried out on a few normal subjects and on a series of epileptics. EEG recordings were taken with an 8-channel Grass apparatus, but no automatic analyzer was used. In every experiment, recordings were made for 60 continuous minutes. Adrenochrome was given intravenously in dosage from 10-50 mgms.

As adrenochrome rapidly loses its properties by auto-oxidation, it must be freshly prepared before each experiment (it is made up in 2 cc.'s of distilled water and the solution mixed with the subject's blood in the syringe before injection to prevent severe pain).

**Normal Material.**—A few volunteers received adrenochrome intravenously in 10-25 mgm. dosage. Subjective changes were observed in only a few cases; these took the form of apprehension or foreboding expectations. The heart rate, respiration, and blood pressure remained unchanged and no specific change in the EEG pattern was observed (except for muscle artifacts and a slight increase of fast activity due to apprehension).

**Epileptic Material.**—In our description of epileptics we used the Montreal classification (10), and in the description of the EEG we used Jasper's terminology. Our material consisted of 21 cases of epilepsy (adrenochrome was not given to those below the

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age of 10). The dosage was 10, 25, and 50 mgms. Subjective changes were reported only after 50 mgms. of the drug (8 cases). All felt slightly sleepy or drowsy and 2 experienced a feeling "as if a seizure were developing." In 2 other subjects, feelings of strangeness and unreality were reported with a focus in a temporal lobe.

No change was noted in heart rate, respiration, or blood pressure. Generally about 10-15 minutes after adrenochrome was injected the following changes were noted in the EEG: (1) a decrease in the basic frequency (2/second), and approximately a 30% decrease in voltage; (2) an increase of the dysrhythmic pattern and hypersynchrony; (3) an increase in the activity of the focus.

Our material consisted of 3 cases of centrencephalic epileptics (Group 1), characterized by short lapses of consciousness and typical 3/second spike and wave activity in their EEG. Our adrenochrome study, on subjects with this type of epilepsy, was not extended as they were below the age of 10. There were 5 cases with grand mal (Group 2) without clinical and EEG focus and 21 cases of focal-cortical seizures (Group 3) (Table 1).

In Group 2, adrenochrome produced a marked increase of dysrhythmia as well as

increased sensitivity to hyperventilation, and in 2 cases there was a lowering of the photo convulsive threshold. In Group 3 the following was observed: (1) an increase of voltage and decrease of frequency as described above; (2) an increase of the dysrhythmic pattern; (3) in 4 cases a spontaneous increase in irritability of the focus; (4) in all cases a marked increase of the focal activity of hyperventilation along with a spread of pathological activity in the opposite homologous cortical area.

Nicotinic acid was again given to normal volunteers and to all epileptics tested. In the normal volunteers the drug produced a slight increase in frequency and a decrease in voltage. When given intravenously the electrical change appeared somewhat later than the flush on the face. Our experiments showed the same results as described by Darrow (12). We are unable to state definitely the minimum dosage of the drug required to produce electrical changes, but it seems evident that a fairly high blood level of niacin is necessary to elicit the effects described above. (Investigations are in progress to determine the required blood level of the drug). From our epileptic material the drug seemed to reverse the effect due to adrenochrome: (1) in every case there was a shift in frequency to the fast side with

TABLE 1

	No. of cases	Routine EEG findings	Changes in EEG on adrenochrome	Changes in EEG on nicotinic acid
Group 1	3	a. High voltage, diffuse abnormality b. Bursts of hypersynchronous 3-per-sec. spike and wave activity	Not given	a. Slight decrease of diffuse abnormality b. No change in the paroxysmal 3-per-sec. activity
Group 2	5	High voltage, diffuse and paroxysmal abnormality: a. Bilateral hypersynchrony b. Diffuse high voltage 5-per-sec. activity	Increases of diffuse and paroxysmal abnormality with increased amplitude	Decrease of diffuse and paroxysmal abnormality, increase in frequency and decrease in amplitude
Group 3	15	a. Focal activity showing spike, sharp wave and irregular delta in all cases b. Diffuse bilateral slow activity in 10 cases	a. Spontaneous increase of focal activity in 4 cases. Hyperventilation produced an increased focal sensitivity in all cases b. Increased diffuse bilateral abnormality in all cases	a. Slight decrease of the intensity of focal activity b. Marked decrease of the diffuse bilateral abnormality in all cases

lowering of voltage; (2) the paroxysmal pathological discharge was of lesser intensity but the activity of the focus itself showed no definite change. On grouping our material (Table 1), Group 1 showed only a slight change in the EEG and the 3/second spike and wave discharge on hyperventilation was unaltered; in Group 2 the paroxysmal activity was much less intense and the basic pattern became more regular; in Group 3 the generalized dysrhythmia was much less intense, but the spontaneous focal-cortical activity remained unchanged and was much less sensitive to hyperventilation.

Subjectively most of the patients felt more alert and fresh after receiving the drug. This phenomenon was clearly observed in patients who had been institutionalized for years because of their epileptic deterioration.

We gave 300 mgs. nicotinic acid intravenously to a case of narcolepsy with the result that no spontaneous sleep activity occurred while recording. Before the drug, the patient showed sleep activity every 2-3 minutes (Figure 1, a and b).

#### CASE PRESENTATION

M.F.M., 16-year-old female, her history marked by mixed petit mal and grand mal seizures, in the past few years has had no seizures, but showed a change in her behavior, became depressed, irritable, and had periodic feelings of estrangement. Lately she has developed "spells" in her stomach—a sudden discomfort which moves upwards and leads to intensive itching of her nose. This is accompanied by anxiety and a feeling of estrangement. At times microptic, macroptic, and metamorphoptic hallucinations and illusions appear. Sometimes she has dreamlike states. On neurological examination no pathological disturbance was found. Her EEG showed a very marked left anterior temporal focus, characterized by high voltage slow waves with an occasional "saw-tooth" appearance and single spikes (Fig. 2a). Her left temporal focus was sensitive to hyperventilation with a spread over the homologous area. Fifty mgms. adrenochrome were given intravenously. After 10 minutes the patient experienced a sudden feeling of estrangement and fear, with itching of her nose. There was an increase of pathological activity in her focus as well as an appearance of generalized dysrhythmia (Fig. 2b). After 400 mgms. of nicotinic acid given intravenously (1 cc./30 seconds) the dysrhythmia disappeared, and her focus became less intensive although still actively firing (Fig. 2c). Her subjective estrangement disappeared.

M.M., a 40-year-old, male Indian, in the past 15 years has had grand mal seizures without any

focalities. His neurological examination revealed no pathological disturbance. Mentally he was dull. The EEG showed a well-circumscribed focus in his left anterior temporal area, characterized by bursts of single spikes and slow sharp wave activity (Fig. 3a). On hyperventilation there was an increase of the above-mentioned abnormality. Fifty mgms. of adrenochrome given intravenously resulted in increased activity (Fig. 3b); 1 gram of nicotinic acid given orally and 200 mgms. given intravenously restored the normal pattern of the right side with a decrease of pathological activity on the left. Hyperventilation produced little increase in his pathological focal activity (Fig. 3c).

S.D.—This 16-year-old girl was referred to our outpatient department because of her "spells." These were partly of grand mal type preceded by (1) a feeling of impending disaster or joy, (2) *déjà vu* experience, (3) a feeling of unreality. She has a history of asthma. Neurological and air studies were essentially negative. Her EEG showed right-sided anterior temporal focus with generalized dysrhythmia (Fig. 4a). After receiving 50 mgms. of adrenochrome her focal activity increased markedly with an increase of the generalized dysrhythmia (Fig. 4b). After nicotinic acid, the basic rhythm became more regular, and her focal activity became less intense (Fig. 4c).

J.B.—This 16-year-old female was admitted for investigation of her seizures. Her history revealed a prolonged and dry birth, whooping cough, and measles. At age 11 she fell from a hay loft (15 ft.) and lost consciousness for a few seconds. A few months later she experienced her first seizure. Her aura is characterized by a sudden feeling of strangeness and change in her environment with pain and numbness in her right arm. Her seizures began with flexion and abduction in the right wrist and elbow followed by loss of consciousness and tonic-clonic movements. Her neurological and air studies were essentially negative. Her EEG showed symmetrical slow activity bifrontally (Fig. 5a) which markedly increased after 25 mgms. of adrenochrome (Fig. 5b). After 200 mgms. of nicotinic acid intravenously there was a generalized decrease in voltage, the basic rhythm became regular, and hypersynchrony could not be seen.

D.J.W.—This 18-year-old female had her first epileptic seizure at the age of 17 and since then has them at regular intervals—2 or 3 per month. They begin with a buzzing noise on the left side; everything then seems far away; "things are smaller than they are." This is followed by loss of consciousness and tonic-clonic movements. According to her sister her head turns to the right side. Neurological and air studies were negative. Her EEG showed a left-sided anterior temporal focus with generalized abnormalities (Fig. 6a). After 50 mgms. of adrenochrome there was a very marked increase of hypersynchrony (Fig. 6b). After 300 mgms. of nicotinic acid intravenously her basic rhythm became regular and no hypersynchrony could be seen. The only pathological feature evident was a continuous slow activity of medium voltage in the left frontal area (Fig. 6c).



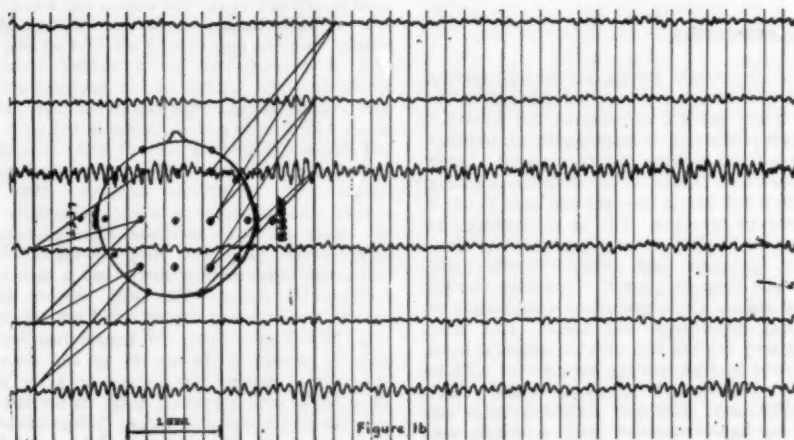
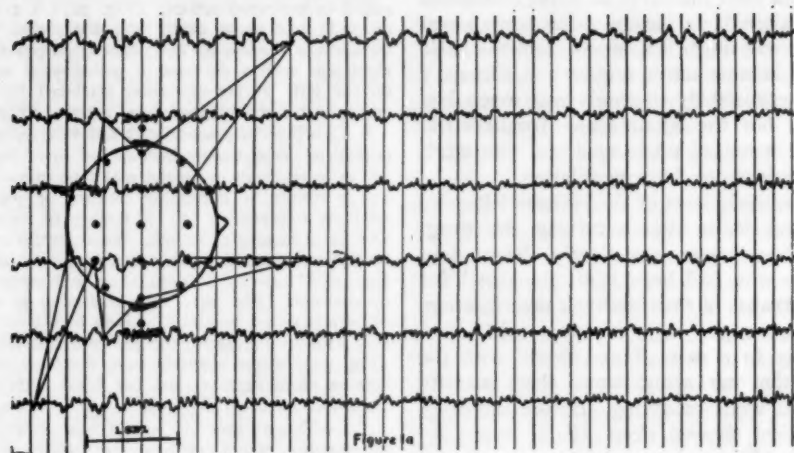


FIG. 1.—A case of narcolepsy: (a) before injection with 300 mgs. nicotinic acid; (b) after injection.

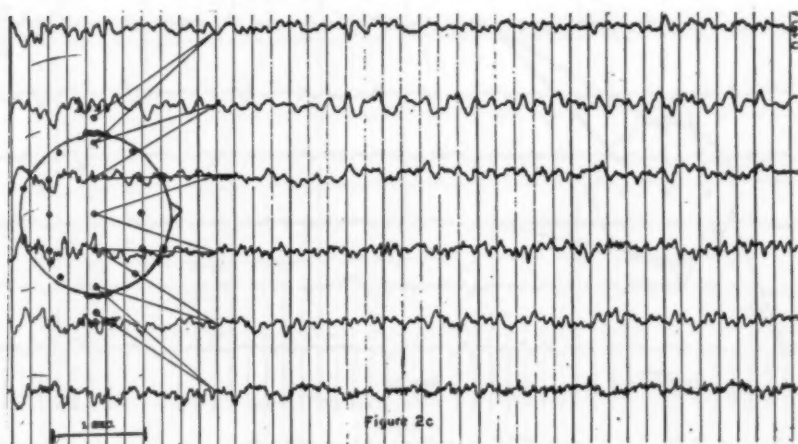
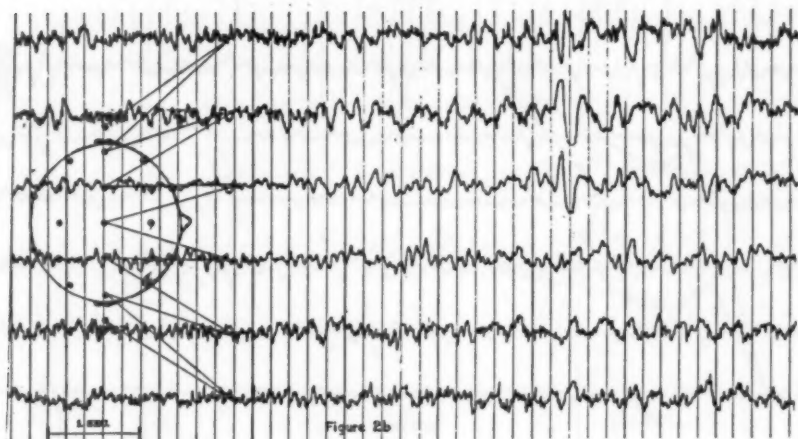
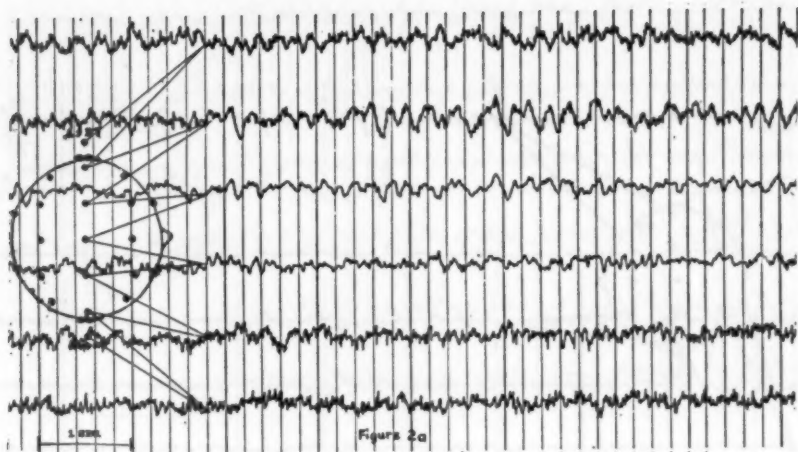


FIG. 2.—Subject M.F.M. (a) Control EEG recording; (b) 10 min. after receiving 50 mg. adrenochrome; (c) 15 min. after receiving 400 mg. nicotinic acid I.V.

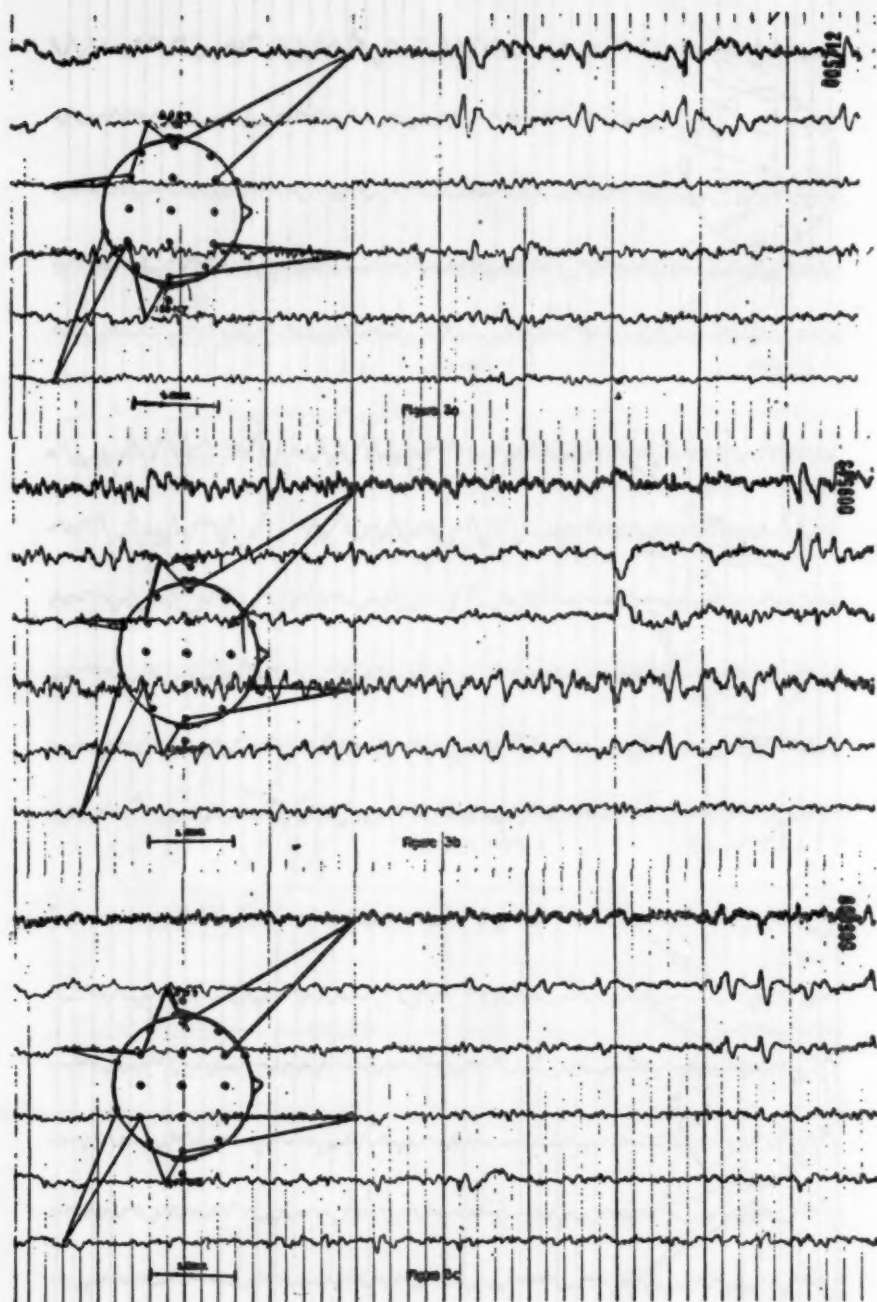


FIG. 3.—Subject M.M. (a) Control EEG recording; (b) 10 min. after receiving 50 mg. adrenochrome; (c) 15 min. after receiving 1 gram of nicotinic acid orally and 200 mg. I.V.

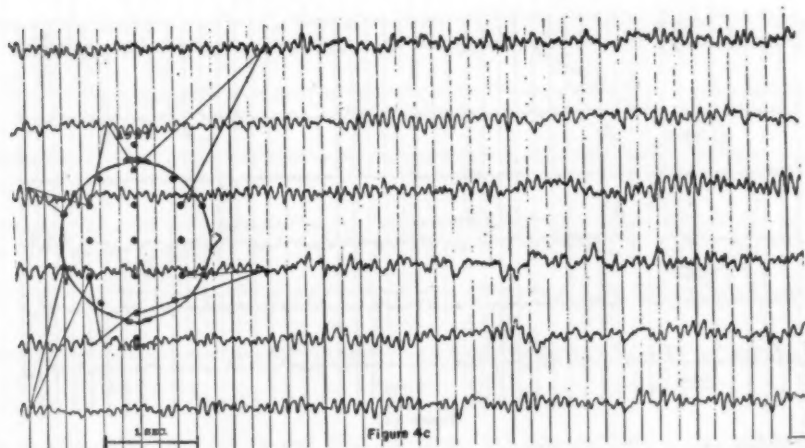
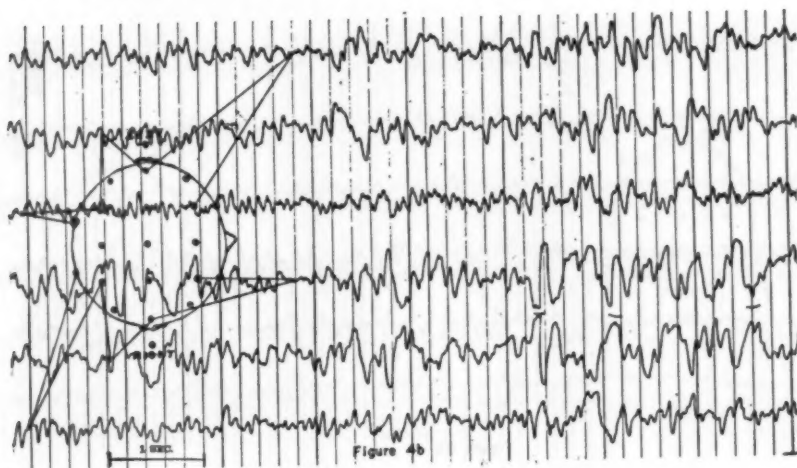
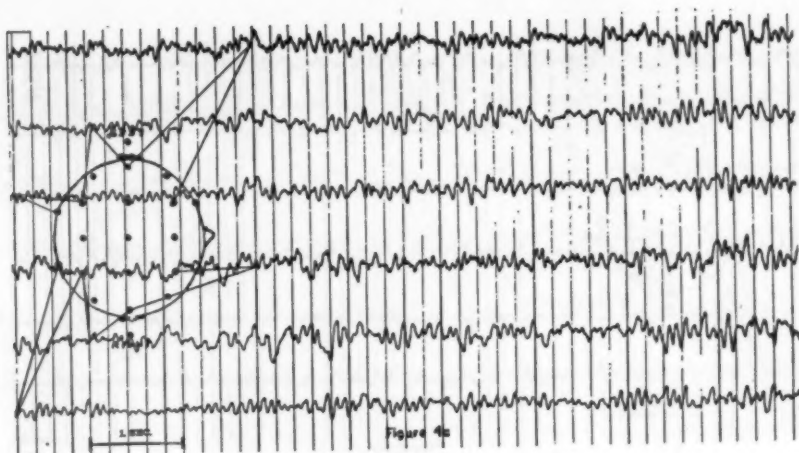


FIG. 4.—Subject S.D. (a) Control EEG recording; (b) 10 min. after receiving 50 mg. adrenochrome; (c) 15 min. after receiving 200 mg. nicotinic acid I.V.



Figure 5a

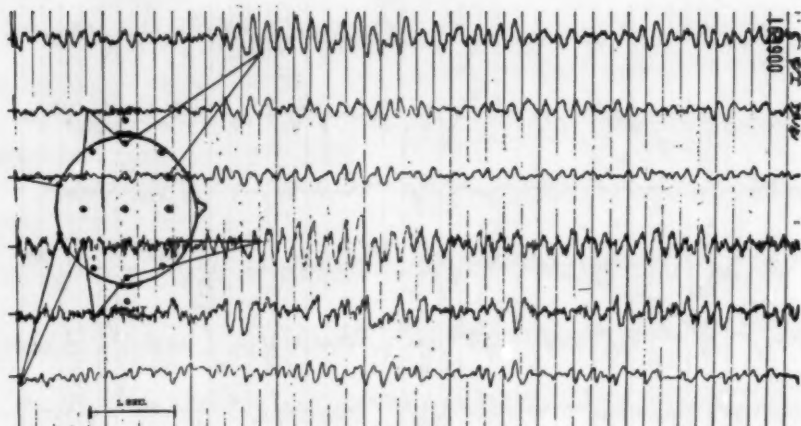


Figure 5b

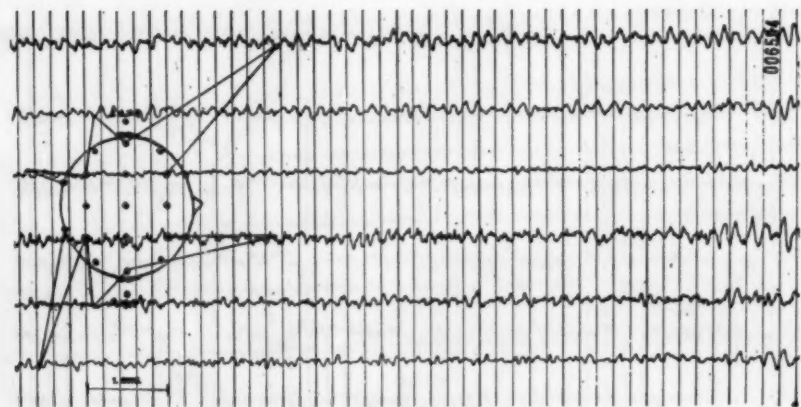


Figure 5c

FIG. 5.—Subject J.B. (a) Control EEG recording; (b) 10 min. after receiving 10 mg. adrenochrome.



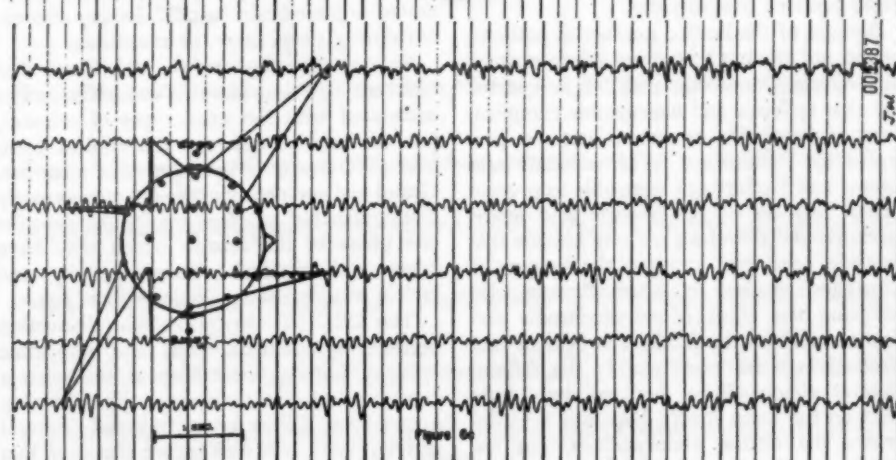
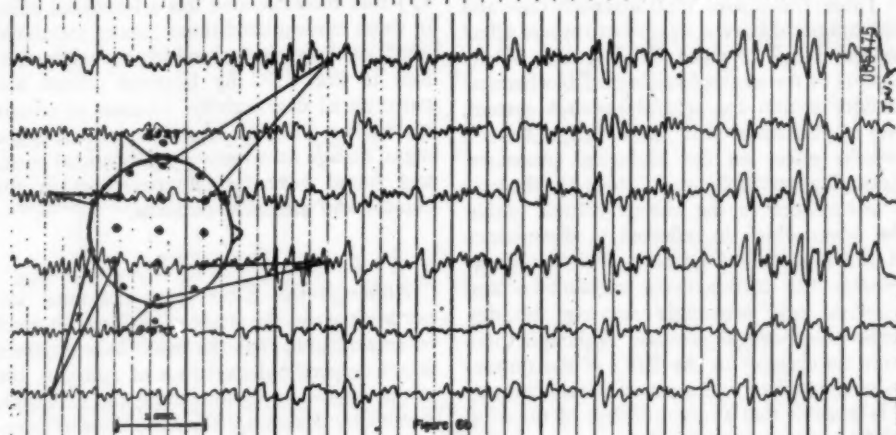
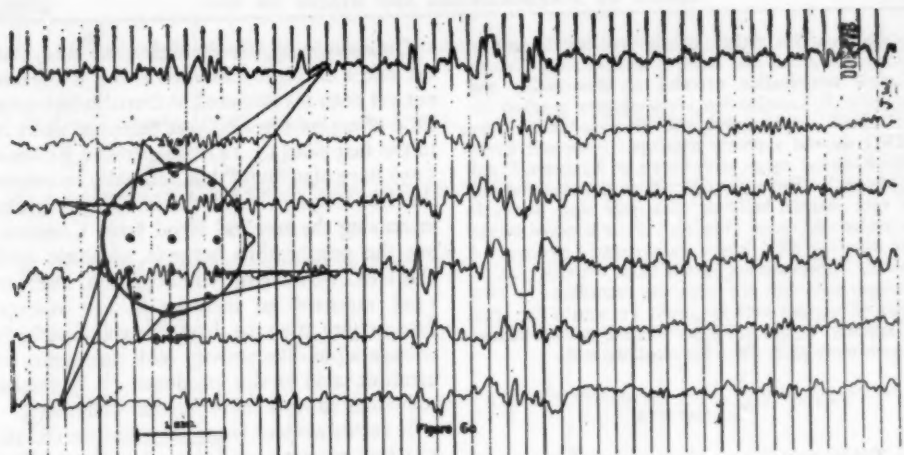


FIG. 6.—Subject D.J.W. (a) Control EEG recording; (b) after receiving 50 mg. adrenochrome, (c) after receiving 300 mg. nicotinic acid I.V.

A. Th.—This patient, hospitalized for the past 20 years, has grand mal seizures without aura. Neurological examination revealed no abnormality and psychiatric examination revealed very marked intellectual deterioration and slowing down. His EEG showed a poorly developed basic activity in the 8/second range with bursts of hypersynchrony (Fig. 7a). Ten mgms. of adrenochrome produced a very marked build-up with huge slow waves in the 1/second range (Fig. 7b). Four hundred mgms. of nicotinic acid given intravenously produced a marked improvement in the basic activity, and, in comparison with the previous recordings, a long run of normal activity could be seen (Fig. 7c). Clinically, it was apparent that the patient became much more alert after the nicotinic acid.

#### COMMENTS

Table 1 indicates that adrenochrome and nicotinic acid have a somewhat reverse effect on the EEG. As the EEG is one of the aspects of the neurophysiological-biochemical activity within the central nervous system, we may assume that these drugs have a reverse effect on the biological processes. According to Gibbs and Gibbs(5), there is a disturbance in the energy release within the brain which is reflected in disturbance of the biochemical process. Adrenochrome inhibits the carbohydrate metabolism and nicotinic acid seemingly restores the disturbed biochemical process. Although there were no changes in the EEG of the normal volunteers after 10 mgms. of adrenochrome, it is possible that a larger dosage of the drug might produce irregularities.

Because of the limited number of patients, we did not attempt to prevent adrenochrome after-effects with nicotinic acid. It is reported (9) that epileptic and neuropathic complications of isoniazide treatment can be prevented by Pyridoxine. With nicotinic acid Agnew and Hoffer(44) were able to prevent certain features of the lysergic acid model psychosis and therefore we may assume that nicotinic acid may prevent the enzymatic disturbance caused by adrenochrome. On analyzing the effect of adrenochrome and nicotinic acid on the EEG, it is evident that adrenochrome increases mainly the diffuse, bilateral abnormalities, but has very little effect on the focus itself. Nicotinic acid decreases the diffuse abnormalities, but the activity of the focus itself decreases only slightly.

On examining the literature, we find that the effect of adrenochrome on the EEG has not yet been investigated. Adrenalin has very little effect on the EEG; it seems to shift it to the fast side(13, 14). Jasper and Erikson (15) have also found that adrenalin increases the fast frequencies of the EEG by slightly increasing the cerebral blood flow. Concerning the relationship between nicotinic acid and EEG we have little information. Darrow (12) reported an increase in low voltage fast activity after the drug. In children, high voltage alpha-like activity with 50 mgms. of nicotinic acid orally produced "a dramatic reduction toward more normal behavior."

It seems evident from the literature(8, 16, 17) that nicotinic acid has a beneficial effect in those clinical conditions (toxic, metabolic, nutritional psychoses) which are characterized in the EEG by bilateral diffuse and paroxysmal slow activity. In cases of seizure activations we gave nicotinic acid in 2-300 mgm. dosage intravenously in postictal coma, and found very fast improvement in the clinical and electrical patterns.

#### DISCUSSION

Although the EEG plays the most important part in the study of epilepsy, it must be remembered that the interseizure pattern is not a specific disturbance of epilepsy, but can be caused by various toxic and metabolic factors, by postencephalic conditions or deep-seated organic brain lesions. It may also occur during sleep, fever, or anesthesia.

Several investigators(18, 19) found that different electrical patterns are more or less associated with the main types of seizures, but the relation between the clinical form and EEG features is not always clear-cut. Grand mal and petit mal are not always easily distinguishable as they may only describe one phase of the seizure which may have begun with bilateral 3/second activity, and end in a typical tonic-clonic seizure(10).

The EEG has emphasized the distinction between symptomatic-focal and idiopathic epilepsy, but one must keep in mind that a focal-cortical disturbance may later develop a non-focal diffuse or paroxysmal bilateral activity(20). It is well known that even in a well-localized cortical focus, we may find spontaneous or activated bilateral abnormali-

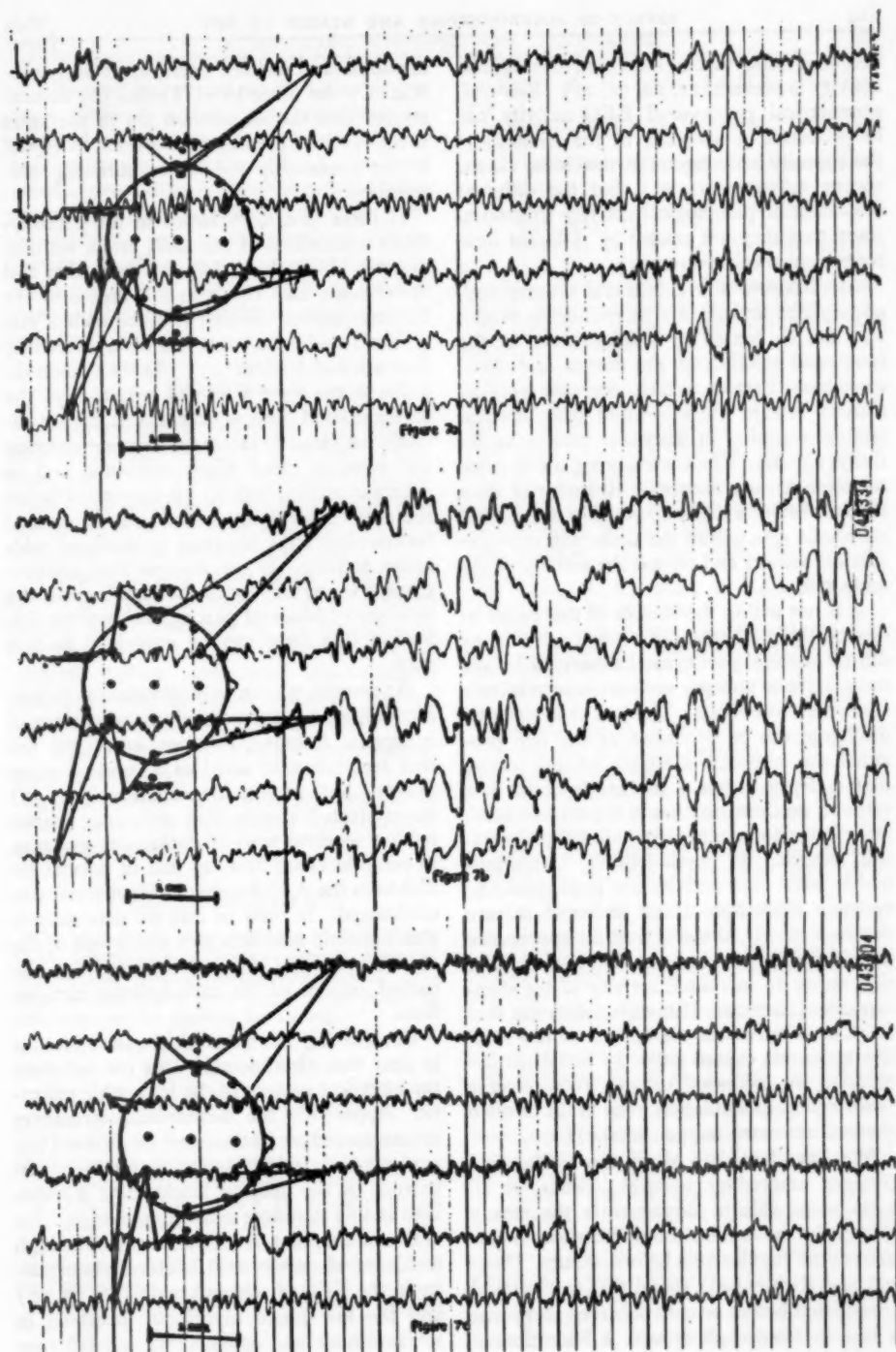


FIG. 7.—Subject A. Th. (a) Control EEG recording; (b) after 10 mg. adrenochrome; (c) after 400 mg. nicotinic acid I.V.

ties, suggesting that an increased diencephalic activity is present (21, 10, 11, 22). Bilateral, symmetrical paroxysmal EEG activity can be produced by spreading of the stimulus to the opposite homologous cortical area, via the corpus callosum (10, 23), but the different responses of pathological activity to arousal show that they are caused by different neurophysical mechanisms (24).

As discussed above, bilateral symmetrical, paroxysmal activity are of mid-brain origin, and can be caused by a disturbance in the functional relation of the cortex and diencephalon. Factors which decrease cortical activity release the mid-brain from cortical control resulting in increased mid-brain activity (25, 26). The cortex being much more sensitive to any metabolic disturbance than the mid-brain, a slight decrease in the normal metabolic rate within the brain will decrease cortical activity and release the activity of the mid-brain.

It is not within the bounds of this paper to discuss the mechanism of evoked seizures, as diffuse bilateral paroxysmal abnormalities are not specific to epilepsy and can be seen in several other conditions. The fact that epileptic discharge can be produced in any one provided the stimulus is strong enough shows that epilepsy is not a disease in itself, but rather a uniform reaction of the central nervous system to certain stress situations, physical, clinical, or psychological. Conditions which favor slow activity are predisposed to epileptic discharge. If an activated seizure develops slowly, there is first an appearance of bilateral, high voltage paroxysmal activity suggesting an increased activity of the diencephalic structures. Himwich points out that in all so-called shock therapy, there is a relative anoxemia caused either by metabolic depression or increased oxygen requirements (on electrical stimulation even brain *in vitro* showed increased oxygen intake) (27).

We have few data about the biochemical changes preceding seizures. Gibbs, et al. (28) were able to demonstrate the steady change in the carbon dioxide content measured in the jugular vein before seizure. Dawson and Richter (29) found that in electrical stimulation before seizure occurred there was a rise in hexose phosphate, a liberation of

ammonia and marked decrease of acetylcholine. On the other hand, Torda (30) demonstrated that the ammonium ion in the brain is the result of increased cerebral activity and is not necessarily the factor initiating convulsions.

If there is a disturbance in the carbohydrate metabolism of the adult brain, we may assume, although we have no data, that this disturbance may result in a decrease of the intrinsic carbon dioxide content with a rise of lactic acid (the latter has been proved by Dawson and Richter (29)). As the carbon dioxide is the most powerful regulator of the cerebral blood flow, a decrease in carbon dioxide may lead to increased vaso-constriction and possibly cause slight anoxemia and so decrease cortical activity [in hyperventilation there are clinical signs of decreased cortical functioning (26)] resulting in increased mid-brain activity. If we assume that adreno-chrome is an inhibitor of the Krebs' cycle, the increase of bilateral paroxysmal activity following this drug can be explained in this way.

At present, we can only speculate as to how nicotinic acid acts on the electrical pattern of epileptics. Lehman, Reitman, et al. (33) felt that nicotinic acid acted as a cerebral vasodilator, but recent investigations (34, 35) demonstrated clearly that this drug causes neither cerebral vaso-dilatation nor increase, in cerebral blood flow, so that in normal individuals the A.V. oxygen difference remains unchanged. In view of this we may assume that nicotinic acid acts as a coenzyme in the Krebs' cycle and, as such, restores the disturbed pattern of the carbohydrate metabolism.

Considering this theoretical assumption, it is clear that nicotinic acid does not influence the electrical pattern of the idiopathic epileptic. Apparently the diencephalic structures are oversensitive in the latter condition (low synaptic threshold, Gastaut): a minute imbalance of the delicate biochemical metabolism is able to induce seizure discharge.

One more point may be relevant. The high incidence of paroxysmal bilateral abnormality in the EEG of schizophrenics (36, 37, 38) and the low photo myoclonic threshold in schizophrenia and epilepsy (39, 40, 41) may



be explained in two ways: (1) the paroxysmal bilateral activity may represent an increased diencephalic reaction as a homeostatic principle(42, 43), and (2) as mentioned above, Osmond, Smythies, and Hoffer suggest that adrenochrome or a similar substance plays a part in the genesis of schizophrenia. If there is an adrenochrome-like substance present in the body of schizophrenics, the above-mentioned EEG disturbances may result.

### SUMMARY

1. Adrenochrome in dosages of 10 mgms. does not change the EEG of normal volunteers, but in 10, 25, and 50 mgm. doses increases the bilateral paroxysmal abnormalities in the EEG of epileptics, but has very little effect on the cortical focus itself.

2. Nicotinic acid given orally or intravenously in normal volunteers showed a slight shift in EEG frequency to the fast side; in epileptics the drug considerably decreases the bilateral diffuse paroxysmal abnormalities (except in true idiopathic epilepsy), but has very little influence on the focus itself.

3. The mode of action of the above-named drugs is discussed.

4. A tentative explanation is offered as to the genesis of the paroxysmal EEG disturbance in schizophrenia.

### ACKNOWLEDGEMENTS

The photographs were prepared by Mr. Harry Wood, photographer, Regina General Hospital. Dr. Osmond, Superintendent, Saskatchewan Hospital, Weyburn, was most helpful in providing us with some of the patients for testing.

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## CHANGES IN BLOOD PRESSURE WITH ELECTROSHOCK THERAPY IN A PATIENT RECEIVING CHLORPROMAZINE HYDROCHLORIDE (THORAZINE)<sup>1</sup>

DANIEL M. WEISS, M.D.,<sup>2</sup> BOSTON, MASS.

Chlorpromazine hydrochloride (Thorazine, SKF 2601-A) was first clinically investigated in France by Delay and Deniker (1, 2, 3), where it was known as Largactil, 4560RP, Megaphen, or Ampliactil. It has been advocated as a sympathetic depressant (4) and as a strong sedative for the symptomatic control of severe agitation and acute anxiety in the psychoses and in psychoneurosis (5). The current literature indicates it has acquired widespread use in psychiatric disorders. In spite of this use however, we have found in the literature no report of combined electroshock-thorazine therapy.

Since we have recently had a patient who suffered a disturbing vasomotor collapse when electroshock had to be superimposed on chlorpromazine hydrochloride treatment as a life-saving measure it seems desirable to report the case for the information of others and as a warning.

GEK, a 47-year-old white married male, was admitted for the fourth time to the Boston V.A. Hospital because of a recurrent attack of manic-depressive psychosis, manic phase. The first 2 episodes ended in remissions with the aid of psychotherapy, and the third episode was terminated with psychotherapy, aided by 7 electroshock treatments. Because of an acute exacerbation he was readmitted to the hospital July 13, 1954. On admission he was confused, partially oriented as to time and place, well oriented as to person. Attention span was very limited, and insight and judgment were considered to be very poor. The patient was very demanding and belligerent, paced the ward restlessly, was hyperirritable, showed increasing confusion and marked flight of ideas, and became insomniac. Seclusion was necessary because of the overactivity and acute regres-

sion in the form of soiling and stripping himself. He began to show signs of physical exhaustion, and it was decided to use chlorpromazine hydrochloride, since this appeared to be the sort of case in which the drug has been claimed to be most valuable.

Dosage was given as recommended: On July 21, 1954, he was given a total of 75 mgm of chlorpromazine hydrochloride, intramuscularly, in divided doses. The patient refused oral medication the next day, so the drug was again given intramuscularly in the amount of 100 mgm. Thereafter medication was given orally, with gradual daily increases up to a maximum of 450 mgm daily by mouth. In addition, the patient was seen in daily interviews for psychotherapy. However he continued to show signs of regressed behavior and it was necessary to keep him in seclusion for the greater part of each day. Speech was almost constant, though almost unintelligible at times, and sleep came only in short naps. Food intake was minimal, flight of ideas was increased, as was the combativeness. The patient appeared very exhausted physically, and we decided that he had had an adequate trial of the drug and that it was not controlling the patient. Since exhaustion was apparent and we were concerned that the patient might die, it was felt that electroshock had to be instituted as a life-saving measure. We thought the drug should be decreased as quickly as possible. The literature was searched in order to determine the effect of electroshock therapy on a patient who was receiving thorazine, and no similar case was found. Since statements in the literature were construed to mean that the drug should not be discontinued precipitously, even though no reason for this was given, we reduced the drug by 50 mgm and the first treatment was given with the patient having had 400 mgm orally.

Treatment was given with the Reiter machine, model CW46J. Blood pressures were carefully followed throughout (Fig. 1), and

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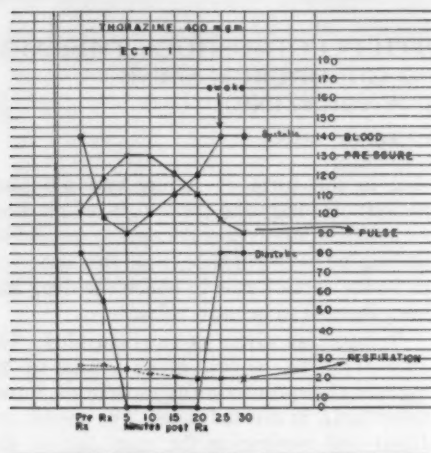


FIG. 1.

after an initial post-treatment rise, the pressure fell very quickly, with the diastolic pressure reaching the zero level. As the pressure fell the patient became cyanotic, pale, and perspired profusely. The pulse was rapid and thready, but returned to normal coincident with the blood pressure. Respirations were shallow but similarly returned to normal. Within 30 minutes the pressure had stabilized to the patient's baseline level. While the acute drop in blood pressure was disturbing, nevertheless the patient recovered without other incident, so we decided to continue treatment while paying careful attention to the pressures. The drug was decreased to 350 mgm, and vital signs were observed after the second treatment. Again (Fig. 2) the diastolic pressure fell to the zero level, but recovered more quickly than after the first treatment. On the third day only 250 mgm of the drug were given, and the next treatment was followed by a drop in pressure of lesser degree. During the next few days the drug was discontinued, and succeeding electroshock treatments were followed by average or normal blood pressure changes. The patient had a total of 15 electroshock treatments, and there was a normal blood pressure curve after the last one (Fig. 3). Following the treatment the patient showed marked improvement, and was in good contact with his surroundings. He was less irritable, was no longer confused or

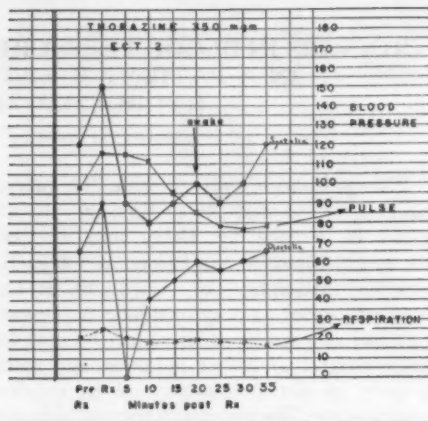


FIG. 2.

denudative, was well oriented in all spheres, and the flight of ideas was not evident. He took part in all ward activities, and seclusion was unnecessary.

#### SUMMARY

A case is presented which did not respond to the administration of chlorpromazine hydrochloride in the level of 450 mgm daily, but instead seemed to become more regressed and hyperactive.

Electroshock therapy was given as a life-saving measure while the patient was still receiving chlorpromazine hydrochloride in large doses.

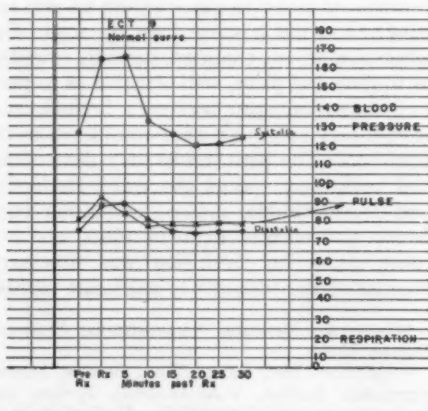


FIG. 3.

A marked fall in blood pressure occurred immediately following the treatment, and a return to the baseline level was seen within 30 minutes. The degree of drop in blood pressure was greater when larger doses of the drug had been received by the patient. This fall in pressure was not present in subsequent treatments, when the patient was not receiving chlorpromazine hydrochloride.

It is felt therefore that the hypotensive action of chlorpromazine hydrochloride is intensified by electroshock therapy. Careful search of the literature has not disclosed a similar case.

#### CONCLUSION

Electroshock therapy should be given with great care to a patient who is receiving chlorpromazine hydrochloride, and then only when

adequate means are available for combatting vascular shock.

#### ACKNOWLEDGMENT

I wish to express my sincere thanks to Dr. Wilfred Bloomberg for his interest in this work.

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## CLINICAL NOTES

### THORAZINE

KENNETH A. SMITH, M.D., HOWARD T. FIEDLER, M.D., AND CHARLES R. YHOST, M.D.  
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Thorazine (Chlorpromazine hydrochloride, S. K. F.), because it has a unique depressive action on the central nervous system, has been found to be of unusual value in the symptomatic control of certain neuropsychiatric disturbances.

At Retreat State Hospital, Hunlock Creek, Pa., 24 patients were given Thorazine, the average dose being 400 mgm. per day given orally or intramuscularly. Of these 24 cases, 2 developed clinical jaundice which rapidly disappeared after discontinuance of the drug. On one of these patients liver function tests and gall bladder studies were normal. Three patients developed an itchy, erythematous, papular eruption on their trunks and upper and lower extremities. The face was spared in both cases and the drug was stopped as

soon as the eruption developed in order to prevent its further extension. Itching was pronounced and urticarial wheals developed after scratching (dermographism). One patient, whose skin was ordinarily more sensitive to the sun, developed marked redness of the face, neck, and dorsal surfaces of the hands and edema developed under her eyes after exposure to the sun and after she had been on Thorazine for about 2 weeks. One patient developed some slight edema of face after exposure to the sun and after she had been on Thorazine for about 2 weeks (photosensitization effect?).

This short clinical note was made just to report some of the clinical manifestations of this new drug and to stimulate further studies along this line.

### ELECTROSTIMULATION IN APNEA AFTER ECT

RUSSELL MEADOWS, JR., M.D., WARREN, PA.

This technique was first used by the writer in a case of apnea of dangerous duration following ECT. The result was quite dramatic. Uniformly favorable results have been obtained by this method in some 45 instances.

It is necessary to distinguish apnea from respiratory obstruction following ECT. In such cases simple measures such as traction on the jaw are usually adequate. In respiratory depression or apnea, the common practice has been to use either artificial respiration or cerebral electrostimulation.

The writer has used the Reiter electrostimulator Model CW-47. For phrenic electrostimulation treatment position number 1 with a current of 2.5 M.A. has been found satisfactory. No appreciable difference was observed in varying the modulation control. Electrodes are placed bilaterally over the motor points of the phrenic nerves about 1 inch above the clavicle and slightly lateral to

the tendons of the sternocleidomastoid muscles. Stimulation is usually applied for a few seconds only since there is an almost immediate inspiration of the abdominal type. Twitching of the cervical muscles occurs but appears to be of no consequence.

This technique was used in one case that was a bad risk for ECT (severe mitral stenosis requiring digitalization and frequent mercurial diuretics). The severe apnea following each ECT was relieved by phrenic stimulation which was also successful in preventing cardiac complication such as pulmonary edema.

It is believed that more than phrenic stimulation alone occurs in this treatment. In the same general area of the phrenic nerve there is also the vagus nerve which has sensory fibers from the pulmonary structures that are undoubtedly in close relation with respiratory centers. It is felt that the



first breath the patient takes after stimulation is due to the phrenic components since abdominal breathing from diaphragmatic contraction occurs. Bombardment of the respiratory centers appears to be a result of vagus stimulation for the continued breathing.

Phrenic stimulation has been used in various complications of bulbar poliomyelitis.<sup>1</sup> However, special electronic equipment is used for this since intermittent electrical current is applied. In comparison, stimulation by the Reiter machine is not as precise and involves a large area of stimulation.

In contrast with cerebral stimulation, va-

gus stimulation appears to be a more direct method of alleviating apnea following ECT. It is postulated that cerebral stimulation affects the respiratory centers more indirectly via the hypothalamus. Although effective, cerebral stimulation appears to act much more slowly. Perhaps one feature of the apnea following ECT is that the respiratory center becomes less sensitive to carbon dioxide levels. Higher levels of carbon dioxide appear to act as a toxic agent causing narcosis of the respiratory center. The application of electrical current by either cerebral or phrenic-vagus stimulation restores the sensitivity of the respiratory center.

This brief note is to call attention to an adjunct in respiratory emergencies following ECT that has been found useful.

<sup>1</sup> Macaulay, John C. Phrenic Stimulation in the Treatment of Acute Bulbar Poliomyelitis. *J.A.M.A.* 155: 541, June 5, 1954.

## CASE REPORTS

### HEMIPLEGIA FOLLOWING ELECTROSHOCK THERAPY

ROBERT E. THOMAS, M.D., BALTIMORE, MD.

Hemiplegia following electroshock therapy has been only rarely reported in literature. Olsen(1) described a 73-year-old man with cardiac complaints who was treated with electroshock because of recurrent depressive episodes. After being given the first of a second series of shock treatments, he developed an immediate unilateral convulsion followed by a transient hemiplegia on the same side. Attributing the atypical convulsion to an epileptogenic lesion, Olsen indicated that in his experience a transient paralysis, corresponding to an epileptogenic focus, following a seizure (Todd's paralysis) was not uncommon. Kaldek(2) subsequently reported on a young woman who suffered a complete spastic hemiplegia immediately after electroshock, the symptoms persisting for several days. This was explained by the author on the basis of the vascular spasm leading to a reversible damage to the motor pathways of the brain. Riese and Fultz(3) recently presented the case of a 44-year-old woman who developed complete flaccid paralysis in all 4 extremities together with areflexia a few hours after her second electroconvulsive treatment. Hallucinations and sudden death followed shortly thereafter. The postparoxysmic paralysis was explained by the authors, on the basis of Jacksonian theory, as "exhaustion" of nerve structures after excessive discharge.

#### CASE REPORT

A 43-year-old, left-handed man with several previous psychiatric hospitalizations for a chronic anxiety state was admitted to the Perry Point V. A. Hospital because of an acute exacerbation of symptoms. After 3 weeks electroconvulsive shock therapy was instituted. Physically he was healthy. No chronic or familial diseases were reported. Blood pressure was 130/85 mm. of mercury. Neurological examination done in conjunction with the routine physical examination showed no pathological signs. Routine serological and other laboratory examinations, chest x-rays, and the electrocardiogram were within normal limits. Skull x-rays

taken during a recent previous admission were negative.

Preceding his first and only electroconvulsive treatment the patient was quiet and outwardly not particularly apprehensive. No preshock medication was given. He was treated with a standard alternating current machine set at 120V for 0.5 sec. duration. An immediate bilateral convulsion developed but it was conspicuously shorter than the average. In the early recovery phase he was excessively active and disturbed for about 10 minutes. Turning in bed, he raised himself on his hands and knees and lunged about as though seeking to slip off a restraining sheet placed across his chest. No motor impairment was noted. Subsequently he became quieter and then lethargic. About an hour after treatment he still had not awakened. When partially aroused for examination, he lay quietly on his back, answered simple questions, and stated that he had no headache.

Neurological examination revealed complete right hemiplegia with flaccid paralysis, facial weakness on the same side, and a corresponding loss of sensation to touch and pain over the right half of the body. No nuchal rigidity was noted and a lumbar puncture was not attempted. There was a persistent deviation of the tongue to the right, and of the head and eyes to the left, of the midline. The development of hemianopsia was not apparent. Speech was thick and swallowing difficult. The blood pressure was then 96/60 mm. of mercury. The deep reflexes were hyperactive, more so on the right, and the abdominal reflexes were absent.

A positive Babinski sign appeared on the right the third day after the injury. On supportive therapy the patient's critical condition gradually improved to the point where he was able to swallow and talk more satisfactorily.

An electroencephalogram obtained on the sixth day following the hemiplegia was influenced by his semicomatose condition. It showed a basic activity of 9-per-second waves. However, numerous outbursts of 7-8-per-second waves occurred and were attributed to the stuporous state. No seizure discharge, focal slowness, or amplitude asymmetry was noted. He did not appear unduly depressed or preoccupied beyond a natural concern regarding his recovery.

After 3 weeks, intensive physiotherapy was started which consisted chiefly of passive exercise and manipulation of the extremities. Slowly the paralysis of the right cheek improved and there developed a slight return of sensation and motor power to the right leg, arm, and fingers of the right hand. About 8 months after the onset of paralysis he was able to walk with the aid of a

cane and brace and was subsequently discharged to home care.

A follow-up communication 16 months after the injury reveals his mind "is as clear as before his illness." The facial muscles are normal and he is ambulatory without aid or support but his right arm and hand remain virtually useless. Some muscular movement of the right arm is possible although he cannot move his hand or fingers separately. A slow return of tactile sensation on the right side is reported. He remains physically healthy and free from bowel or bladder complaints.

#### SUMMARY

1. A case is reported of complete right hemiplegia following the first electroconvul-

sive treatment in a physically healthy 43-year-old man with a psychotic depression.

2. A brief review of 3 somewhat similar cases is reported from the literature.

3. A 16-month follow-up reveals the continued remission of the initial depression and partial recovery from the hemiplegia.

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## CORRESPONDENCE

*Editor, AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: During October 1953 I wrote a letter to the members of our Connecticut Society for Psychiatry and Neurology in which I suggested that the various state hospitals should have their names changed eliminating the word "State." It was my opinion that this descriptive word serves no useful purpose and is antiquated.

Almost 100% of the replies were in favor of this suggestion. The issue was then presented to the Society and will be considered in the future. In the meantime I observed in a news item in the Mail Pouch a proposal from West Virginia similar to mine. I am sure that many others have thought about correcting the appellation of state institutions in the manner described above and it is with this in mind that my letter is directed to the JOURNAL.

LOUIS H. GOLD, M. D.,  
Hartford, Conn.

*Editor, AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: The readers of this magazine will be interested in a publication recently announced by the Group for the Advancement of Psychiatry entitled, *Integration and Conflict in Family Behavior*, GAP Report No. 27.

This report is the first of a series being prepared by the Committee on the Family on the relation between the family and the mental health or illness of the individual. The general question being asked in all the reports is whether one can distinguish various states or conditions in the family which are

either hostile to or promote the healthy adjustment of the individual. This first report proposes a method for dealing with this question. The method consists of defining the social roles of the members of the family in accordance with the values arising from the cultural background of the family. So defined, the roles are then matched and analyzed for the degree of integration or conflict which inheres in them. In this way, family problems can be related rather precisely to cultural conflicts, on the one hand, and to the emotional problems of the individual on the other.

The Group for the Advancement of Psychiatry has a membership of approximately 150 psychiatrists, organized in the form of a number of working committees of about 10 members each, which direct their efforts toward the study of various aspects of psychiatry and toward the application of this knowledge to the fields of mental health and human relations. GAP is an independent group and its reports represent the composite findings and opinions of its members only, guided by its many consultants. Collaboration with specialists in other disciplines has been and is one of GAP's working principles.

Copies of GAP Report No. 27 may be obtained by writing to the Group for the Advancement of Psychiatry, 3617 West 6th Avenue, Topeka, Kansas, and enclosing 50 cents.

WALTER E. BARTON, *President*,  
Group for the Advancement  
of Psychiatry.

### ENGLISH

I would make boys all learn English; and then I would let the clever ones learn Latin as an honour and Greek as a treat. But the only thing I would whip them for is not knowing English. I would whip them hard for that.

—SIR WINSTON CHURCHILL

Every editor knows the trials of reading manuscripts written by persons who had desecrated Sir Winston's whip.

## OFFICIAL REPORTS

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### REPORT OF THE BOARD OF TELLERS

The Board of Tellers met in the office of the Association in New York City, December 3, 1934, at 9:30 a.m., and tallied the ballots for the Proposals to Amend the Constitution, with the following results:

Total Ballots Mailed.....	6,918
Total Ballots Returned.....	3,125
Illegal Ballots .....	3
	Yes      No
Proposal 1 .....	2,913      210
Proposal 2 .....	3,034      89
Proposal 3 .....	3,038      85
Proposal 4 .....	2,731      392
Proposal 5 .....	3,064      59

ARCHIE CRANDELL, M. D.,  
*Chairman.*

EDGAR C. YERBURY, M. D.  
ROBERT S. GARBER, M. D.

AUSTIN M. DAVIES, *Custodian of Ballots.*

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MAD, *adj.*: Affected with a high degree of intellectual independence; not conforming to standards of thought, speech and action derived by the conformants from study of themselves; at odds with the majority; in short, unusual. It is noteworthy that persons are pronounced mad by officials destitute of evidence that themselves are sane. For illustration, this present (and illustrious) lexicographer is no firmer in the faith of his own sanity than is any inmate of any madhouse in the land; yet for aught he knows to the contrary, instead of the lofty occupation that seems to him to be engaging his powers he may really be beating his hands against the window bars of any asylum and declaring himself Noah Webster, to the innocent delight of many thoughtless spectators.

—AMBROSE BIERCE,  
*The Devil's Dictionary*



## COMMENTS

### A WORD OF CAUTION

This brief comment, following the one on the History of Child Psychiatry (Nov. 1953), may not be amiss at this time for it points out some current dangers attending training centers in this field. Third year residents in general psychiatry are now applying for staff positions for next year in child psychiatry.

We feel sure there is hardly a week goes by that a letter does not come over the desks of those of us with administrative responsibilities asking if we know of any psychiatrist, young or old, who has had training in child psychiatry. The general trend is in the direction of filling an opening for which money has been appropriated, and very good money, but inability to find applicants. It is of real concern to the authors of these letters that they have not been able to fill these positions which have been open from one to five years and which in some instances has meant forfeiting an appropriation.

The type of positions referred to ranges from directors of community child guidance clinics and other outpatient facilities for children to heads of inpatient children's services, both public and private.

Most of these requests come from responsible medical people who want a psychiatrist with the full two years of specialized training in child psychiatry in a training center accredited by the American Board of Psychiatry and Neurology. This, of course, must be preceded by at least two years of formal residency training in general psychiatry.

However, while the number of trainees in child psychiatry is slowly increasing they are far too few to begin to meet the demand for their services. Consequently, we hear of instances where young psychiatrists right out of regular residency training with as little as five months' rotation in child psychiatry have applied for some of these openings and have been accepted.

Take, for instance, the responsibility of setting up and directing a residential treatment home for disturbed children (and a

number are in the planning stage today). To be able to assume such responsibility the candidate ought to have at least standard training in child psychiatry if progress is to be made in this difficult field. Unfortunately, psychiatrists have allowed themselves to be drafted into heading up such units with only several months' rotation in the specialty.

Training centers in child psychiatry today are for the most part understaffed with psychiatrists and overburdened with teaching loads to the point where their time with patients oftentimes suffers. Much of this load is often due to hospital residents in psychiatry seeking a glimpse of child psychiatry before taking their Board examinations. While this training need must be and should be met, sometimes this glimpse carries with it an initial rainbow that may take longer study and work in the field to dispel.

It is not uncommon for residents to become a little expansive and euphoric upon going from the heavier pressures of a hospital atmosphere to the relatively permissive low-pressure milieu of the child guidance clinic. This in itself often releases problems in the resident that need to be worked through if he is to receive the most out of his supervised training. Since there is apparently no general agreement or disagreement as to the optimum time a general resident should rotate through a child psychiatry service, more experimental teaching needs to be done in this field. At the present time a rotation of at least six months full time might be suggested as probably satisfying most of those concerned.

However, since there is still some question in the minds of those supervising these rotating trainees concerning the value of short training periods, would it not seem wise for residents on the one hand to have a good understanding of these limitations, and on the other hand if they are going to accept positions in child psychiatry to insist on a longer period of training? It must not be overlooked, of course, that short periods of

training in children's clinics can be a fertile source of recruiting residents into the field of child psychiatry proper. We do not know the facts here, but it might well be that for every resident who accepts a position in the children's field prematurely, another has been stimulated to take the standard two years of

training in child psychiatry that is necessary for a clinic director to have if his clinic is to be approved as a training clinic by the American Association of Psychiatric Clinics for Children and if the psychiatrist is looking forward to becoming eligible for membership in the Academy of Child Psychiatry.

S. S. A.

## LAND REQUIREMENTS FOR MENTAL INSTITUTIONS

The recent discussion in the public press between the state authority and local authority over the need for large amounts of land for the new hospital for mental patients to be established in southeastern Pennsylvania brings to the fore several problems in which both the medical profession and the local communities are interested. In this particular discussion the question revolves around the need for a small *vs.* a relatively large acreage of cultivated land in a potentially suburban community for an institution of 2,000 to 3,000 patients.

We are concerned here primarily with two factors—the problem of occupational therapy and the economic factor of an adequate return in food products from the invested capital in valuable land. Such land will probably cost the state about \$2,000 per acre. Those favoring the large landed institutions maintain that by the use of patient labor the cost of maintenance can be reduced and that cultivation of 500 or 600 acres of land will also give extensive opportunities for occupational therapy. Wernersville State Hospital was established a half century ago for just this purpose—that patient labor should help support and maintain the institution. The results were not only unsatisfactory but practically negligible from the standpoint of farming and efficient results.

All through the practice of medicine we can lay down the principle that the use of the patient for industrial purposes gives relatively negligible results. The cost of supervision and control of these patients, especially if the therapy is to have any value, is much greater than if normal persons were employed. It is true that in a farm community where land is inexpensive there are many opportunities for beneficial occupational therapy, partly productive in nature. However, a relatively small tract of 150 to 200

acres would supply ample ground for the production of garden products, fruits, and other delicacies which would be too expensive to purchase in large quantities on the open market. Large deep-freeze units make this type of patient food supply much superior to the former methods of canning for preserving these products throughout the winter. Bulk staple foods such as grains, milk and dairy products, meat, potatoes and the like may be purchased by contract much more cheaply than they can be produced on valuable land by inefficient patient help. The fact that these active treatment hospitals should be in or near metropolitan centers both for convenience of visiting relatives and the medical staff, as well as to correlate them with general medicine and modern hospital facilities, precludes location in an isolated area as formerly, with related cheap farm land as is found only in such remote areas.

Another fact to consider is that most of the patients admitted to these hospitals come from metropolitan centers, not farming communities, and are unfit for such occupation. All of us who have had experience with this problem have come to the conclusion that the material returns from patient labor is practically nil. The number of able-bodied patients that could be employed is limited, and because of lack of experience and training, and the time and personnel necessary for organization and supervision, the project eventually becomes costly.

In the book now in press, by McCarthy and Corrin, the authors have taken an entirely new point of view in reference to this problem. They contend that the state authority should take into consideration the curability of the acute mental cases, especially the large adolescent group, and stress the need for small hospitals for curable cases in contrast to the present policy of a large

institution for 3,000 or more patients, primarily custodial in care as is now the case in most large state institutions. In the former institution the matter of active and intensive therapy is the main purpose of the institution. These hospitals should be located in the metropolitan centers convenient for both relatives and the medical staff. They should be modern hospitals of approximately 500 beds divided into units of 25 or 30 beds each with a well-trained and properly-compensated psychiatrist in charge. Expert consultants would be available and would be utilized in the study of every case. Modern general hospital and laboratory facilities and personnel would be available for all cases. Here the diagnostic and the therapeutic approach would be essentially medical combined with logical, detailed and unhurried, psychotherapy. Special attention would be given to rest, massage, and all forms of practical occupational therapy. The personality of the patient, the occupational and the home setting would receive special attention. Every case would be treated as a medical case, just as any other sick person—not as a mental case. All would be correlated with follow-up therapy by capable social service workers carried out in the home after discharge. Many cases, under such active and scientific therapy, would be sufficiently recovered to return to their homes in a relatively short period, and when necessary continue treatment on an outpatient basis. All of this would be on a sound medical ground

in a modern hospital and atmosphere without any stigmata of abnormality or aura of the insane asylum. Such a centrally located hospital would also afford excellent facilities for the teaching staffs of the medical schools and advance scientific psychiatry in all of its aspects more than by any other means.

The authors concur with the collaborators of the above-mentioned book, some of whom were members of a committee organized several years ago to study this entire subject, that the need today, in the field of psychiatry, is for small therapeutic units, located in or near metropolitan centers, for active, vigorous treatment of the acute mental cases, especially the adolescent psychotic, with special facilities for outpatient care and treatment, rather than for the construction of more large custodial institutions as in the past. It is generally considered today (see: Bloom, M. T. *Where Mental Patients Rule Themselves*. *Today's Health*, p. 36, Jan. 1954) that over 80% of acute mental illness is amenable to modern methods of treatment with recovery or satisfactory readjustment in the community. When such is the case it would only seem logical that we concentrate all of our attention upon prompt and energetic treatment of the early case and utilize our present institutions for custodial care of cases requiring prolonged treatment, and the chronic and terminal cases. In neither instances are large land areas practicable or necessary.

D. J. MCCARTHY, M. D.

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#### HABIT OF ILLNESS

... The state of those, who, after being released from a long and serious illness, are sometimes touched with fits of fever and slight disorders, and, though they have escaped the grave, are nevertheless disquieted with mistrust, and, though now quite well, stretch out their wrist to a physician and complain unjustly. ... It is not that these are not quite well in body, but that *they are not quite used to being well*.

—SENECA

## NEWS AND NOTES

**DR. COBB VISITING PROFESSOR AT NEW YORK UNIVERSITY COLLEGE OF MEDICINE.**—Dr. Stanley Cobb, Bullard Professor of Neuropathology, Emeritus, Harvard Medical School, gave the fifth annual Leopold Stieglitz Lecture at N.Y.U. College of Medicine, December 15, 1954. The subject of the lecture was "Multiple Etiology in Psychiatry and Medicine." The lecture was given in conjunction with Dr. Cobb's 1-month stay at N.Y.U. College of Medicine in the Leopold Stieglitz visiting professorship.

This visiting professorship was established by friends and patients of Dr. Stieglitz, who now at 86 holds medical license number 1 from the State Board of Regents, dated 1891. It is worth noting that in this first examination held by the Board Dr. Stieglitz received a combined mark (in 5 subjects) of 499 out of a possible 500. Dr. Stieglitz is a graduate in medicine from Heidelberg University.

**EMERGENCY COMMISSIONED RESERVE, U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE.**—A major expansion of the Commissioned Reserve of the Public Health Service, Department of Health, Education, and Welfare, as a national defense measure has been announced by Surgeon General Leonard A. Scheele. The Service expects to commission an additional 2,000 reserve officers by June 30, 1955, and present plans call for the commissioning of another 3,000 officers during the 1955-56 fiscal year.

The Service has been assigned extensive new defense responsibilities by the Federal Civil Defense Administration. In addition to building up the Commissioned Reserve to emergency strength, the Service is stepping up research in disaster health problems and is developing a program to reinforce state and local health departments in time of national crisis.

Officers in the emergency reserve may request active duty at any time and will be considered for available assignments. The emphasis initially will be on the commissioning of physicians, dentists, sanitary engineers, nurses, particularly physicians. In the near future, the Public Health Service will train officers of the emergency reserve

in the health problems associated with atomic, biological, and chemical warfare and other national emergencies.

**THE LESTER N. HOFHEIMER AWARD.**—The estate of Lester Hofheimer in May 1947 contributed the sum of \$25,000 to The American Psychiatric Association to provide an annual award for an outstanding research contribution in the field of psychiatry or mental hygiene.

Entries for consideration of this award should be in the hands of the Hofheimer Prize Board no later than March 1, 1955. The Hofheimer Prize Board consists of 8 Fellows and Members of The American Psychiatric Association. Therefore, 8 copies of the work to be considered should be submitted to Dr. Harold G. Wolff, chairman of the Board, at The New York Hospital, 525 East 68th Street, New York 21, N. Y.

The Hofheimer Prize is awarded each year at the annual meeting of The American Psychiatric Association, in the amount of \$1,500, to a citizen of the United States or Canada, not over 40 years old at the time of publication, or submission for publication, of his contribution. The award shall apply only to work published within 3 years prior to the date of the award. The Board may, at its discretion, omit the prize for any one year, but the making of the award shall not be omitted for any two successive years.

**TRAINING IN GROUP DEVELOPMENT.**—Based upon 9 years of pioneering research and experience in the relatively new field of training leaders in group organization, the National Training Laboratory in Group Development will hold two 3-week laboratory sessions at Gould Academy, Bethel, Maine, June 19 to July 8, and July 17 to August 5, 1955. Approximately 125 applicants will be accepted for each session. Persons interested in a training, consultant, or leadership capacity in any field are invited to apply.

The NTLGD is sponsored by the Division of Adult Education Service of the NEA and by the Research Center for Group Dynamics of the University of Michigan, with the cooperation of faculty members from Boston University, the universities of California,



Chicago, and Colorado, Cornell University, the University of Illinois, The Ohio State University, the University of Texas, Wayne University, Grinnell College, Michigan State College, Teachers College at Columbia University, and other educational institutions.

For information address National Training Laboratory in Group Development, 1201 Sixteenth St., N. W., Washington 6, D. C.

**RETIREMENT OF DR. FERRARO.**—Dr. Armando Ferraro, principal research scientist in neuropathology at the New York State Psychiatric Institute, retired from his position as of November 30, 1954. Dr. Ferraro served the New York State Department of Mental Hygiene without interruption in this position since October 18, 1926.

A farewell party was given in his honor on November 24 by the employees and his colleagues on the staff of the Psychiatric Institute who presented him with a gift of airplane luggage as a token of appreciation and remembrance.

#### PSYCHOANALYTIC TRAINING AT MCGILL

—Dr. D. Ewen Cameron, chairman of the department of psychiatry, McGill University, announces the establishment of a training course in psychoanalysis as a service in his department. This course will be under the direction of Dr. W. C. M. Scott who has been appointed associate professor of psychiatry, assisted by Drs. Johann and Gottfriede Aufreiter, formerly of Vienna. Dr. Scott, a graduate of the University of Toronto, has lived for many years in London, England, where he trained at the National Hospital and the Institute of Psychoanalysis. He was lately chairman of the board of directors of that Institute.

Applicants for psychoanalytic training address Dr. W. Clifford M. Scott, Allan Memorial Institute, McGill University, Montreal, Que.

**RORSCHACH COURSES, WESTERN RESERVE UNIVERSITY.**—Three separate groups, all under the direction of Marguerite R. Hertz, Ph. D., Associate Clinical Professor of Psychology, will be enrolled for Rorschach courses during June 1955, as follows:

*Workshop I: Introduction to the Rorschach Method.*—Lectures, demonstrations,

supervised training periods, June 13-17, inclusive.

*Workshop II: Intermediate Course in the Interpretation and Clinical Application of the Rorschach Method.*—Lectures, demonstrations, supervised training periods, June 20-24, inclusive; for applicants who have had introductory courses in the Rorschach method or its equivalent. Students in Workshop I may continue with Workshop II.

*Workshop III: Advanced Course in the Interpretation of Rorschach Records of Various Personality and Clinical Groups.*—Participants may submit records for study June 27 to July 1, inclusive. Admission limited to professionally trained persons in psychology, psychiatry, and psychiatric social work who have at least one full year's experience with the Rorschach method.

The fee for each Workshop is 40 dollars. One semester hour is credited for each Workshop in the cases of those who present a transcript of previous college record upon registration. For application forms or further information, address: The Director of Admission, Western Reserve University, 2040 Adelbert Road, Cleveland 6, Ohio.

**SOUTHERN CALIFORNIA PSYCHIATRIC SOCIETY.**—The second annual meeting of the Society was held at the Hotel Statler, Los Angeles, November 6, 1954. Papers were read by Dr. C. H. H. Branch, professor of psychiatry, University of Utah, and Dr. Norman Reider, chief of psychiatry, Mount Zion Hospital, San Francisco.

The following officers for 1955 were elected: Dr. Leo Rangell, Beverly Hills, president; Dr. George Tarjan, superintendent, Pacific Colony, president-elect; Dr. Eugene Pampian Mindlin, director Mental Hygiene Clinic, secretary; Dr. Jack Vatz, Beverly Hills, treasurer. New Council members are Dr. Norman Q. Brill, Dr. Roberta Crutcher, Dr. Lincoln Rahmann, Dr. Charles Tidd.

**ASSOCIATION FOR RESEARCH IN NERVOUS AND MENTAL DISEASE.**—At its thirty-fourth annual meeting, held in New York City, December 10 and 11, 1954, this Association elected the following officers for 1955: president, Dr. J. E. Moore; first vice-president, Dr. John Romano; second vice-presi-



dent, Dr. David Seegal; secretary-treasurer, Dr. Clarence C. Hare; assistant secretary, Dr. Rollo J. Masselink.

The subject of the 1955 meeting, to be held December 9 and 10, 1955, in New York City, will be "The Neurological and Psychiatric Aspects of the Disorders of Aging."

**CARDIOVASCULAR HAZARDS IN ELECTROSHOCK THERAPY.**—L. H. Gahagan (*Cyclopedia of Med. Surg. & Specialties, Rev. Service*, 1954) emphasizes the risks in treating older patients with electroshock. MacLay reported 62 EST deaths in England and Wales from 1947 to 1952. Thirty-five of these were in persons 55 and over, 6 in persons under 35. Thirty-four of the 62 deaths were directly attributed to the cardiovascular system.

Leo Alexander also reported that circulatory disturbances were the greatest hazards of EST.

At least 3 EST deaths due to heart lesions in patients over 50 were reported in the American psychiatric literature during 1953.

Gahagan suggests that in cases of recent myocardial infarction, severe congestive failure, aortic aneurism, complete heart block, and paroxysmal ventricular tachycardia, one should proceed with caution or perhaps delay EST until the CV status is improved. However, considering the risk of suicide in depressive cases, he believes that doubt in such cases "should as a rule be resolved in favor on EST." The use of drugs inducing myoneural blockade (succinicholine and gallamine) is of course recommended.

**PATIENTS IN MENTAL INSTITUTIONS, 1950 AND 1951.**—Compiled from the 25th and 26th Annual Censuses of Patients of Mental Institutions, this 429-page report contains data on patients with mental diseases in state, county, private, Veterans Administration, and general hospitals, and on institutionalized patients with mental defects and epilepsy in public and private institutions during 1950 and 1951.

More complete and greatly expanded than similar previous studies, this report contains 178 statistical tables showing certain characteristics of first admissions, resident patients, discharges, personnel, and expenditures.

Copies of the report may be obtained at 2 dollars a copy from the Superintendent of Documents, Government Printing Office, Washington 25, D. C.

**NEW CONSTRUCTION AT MANHATTAN STATE HOSPITAL.**—A new 1,324-bed medical-surgical building is under construction at an estimated cost of \$10,000,000 on Ward's Island as part of Manhattan State Hospital. It will rise 21 stories.

The corner stone was laid October 21, 1954, in the presence of Governor Dewey. President Eisenhower who was paying a visit to New York City on that day was also present at the ceremony.

Scheduled for construction on the same site are a 960-bed reception building and a 1,000-bed continued treatment building. The total cost of all new construction will be \$43,000,000.

**ROCHESTER STATE HOSPITAL.**—On October 22, 1950, ground was broken on the site of the Rochester (N. Y.) State Hospital for the construction of a 16-story, 1,100-bed medical-surgical building which will cost \$9,000,000.

**THE CHICAGO COUNCIL OF CHILD PSYCHIATRY.**—This Council, a group of child psychiatrists from the midwest, installed new officers recently. They are Dr. Eugene Falstein, president; Dr. Irene Josselyn, president-elect; Dr. George Perkins, secretary-treasurer; and the following as councillors: Dr. Harry Segenreich, Dr. Adrian Vander Veer, and Dr. Robert Koff.

The retiring president, Dr. George Mohr, has gone to Israel to serve as head of child psychiatry in the Lasker Clinic for 1 year.

**DR. BIGELOW RETIRES AS COMMISSIONER.**—On December 30, 1954, Dr. Newton Bigelow resigned from his post at Albany as Commissioner of the New York State Department of Mental Hygiene. Governor Dewey gave Dr. Bigelow high commendation for his efficiency in directing the New York State Hospital Services. Before going to Albany, Dr. Bigelow was director of Marcy State Hospital. He now resumes the headship of that institution.

## BOOK REVIEWS

**PSYCHIATRIC DICTIONARY.** Second Edition. By *Hinsie and Shatsky*. (New York: Oxford Medical Publications, 1953. Price: \$15.00.)

Prior to the time of Dr. Samuel Johnson, dictionaries (other than bilingual dictionaries) were usually limited to listing and defining the words of the learned. In 1775, Dr. Johnson made his own effort to define the entire accepted language of the day. Standard dictionaries have since developed along this line. In addition, special dictionaries have arisen to cover the unusual languages of special fields, especially in scientific areas. The *Psychiatric Dictionary* pretends to be one of the latter, and, in its first edition, actually was; in its second "edition" it most surely is not.

If one accepts the standard definitions of the words, this is neither a second "edition" nor a "dictionary." "Edit" means to revise and prepare for publication. There has been no revision of the first edition, simply its republication in one binding with a supplement, apparently prepared in 1953. Consequently, it is not a dictionary as defined by Webster, "A work of reference in which the words of any province of knowledge are entered alphabetically and defined." True, the words are eventually found in alphabetical arrangement, but in two completely separate and unconnected sections (except for the binding), so that the searcher finds himself jumping from back to front, and back again, wildly seeking the elusive definition. It would have been at least cheaper for the consumer, if not easier on his temper, had the publisher continued to offer the supplement as a separate publication, rather than concealing it in its new binding under the misnomer of a second "edition"; in fact, the two books would have been easier to use than the one.

Failure to edit the first edition, however, has resulted in a much more serious fault in this work, a fault which will render it practically useless for many psychiatrists; except in two areas, many definitions are obsolete, or inaccurate, or both. The first area which seems to be reasonably accurate includes those branches of psychiatry, such as psychobiology, which have added few, if any, new terms to their language since 1940. The second area, as one who has dealt with students and residents since 1945 might suspect, is that of psychoanalysis. The teacher expects students and residents to become preoccupied with psychoanalysis and its words to the exclusion of basic and clinical psychiatry; one expects a less narrow approach from the lexicographer.

Since the first edition was published, this world has indulged in a war of unprecedented scope. Psychiatry was stimulated by this event; psychiatrists participated in the activity on all sides. New ideas were born, old ideas were brushed up and presented as new, and old ideas were presented

simply as old ideas. Many new terms were coined, some good, some bad, some indifferent, but all needing definition and clarification in relation to our previous nosology. However, when one looks (in the Supplement, of course) for "combat exhaustion," "combat fatigue," or "combat neurosis," one finds a single definition common to all: "See shell-shock." This word turns out to be in the first section (non-supplement), and the definition given is taken from a 1936 publication based on the experience of World War I. Thousands of psychiatrists may just as well have stayed at home: their clinical experience is here discarded.

In the Preface to the Second Edition (and to the Supplement), the authors state, "Since the original publication of this Dictionary in 1940, a considerable number of new terms and concepts have appeared in psychiatric literature." They might well have added, "A new psychiatric nomenclature has been written, approved by The American Psychiatric Association, and adopted by the American Medical Association for inclusion in the Standard Nomenclature." A perusal of the "Second Edition" suggests that they did not add this because they were totally unaware of the fact. Not a single one of the new terms introduced in the new nomenclature, such as "acute brain syndrome" and "chronic brain syndrome," is here defined. Perhaps they are bad terms; they nonetheless are deserving of definition if for no other reason than that they are now a part of the official language of psychiatry. No medical records librarian, struggling to understand a half-finished diagnosis and its meaning, will get any comfort from this "dictionary"; the present-day language of clinical psychiatry is not here. Here nosology suffers with libido stasis (see below), and in many psychiatrists anxiety will be provoked.

This nonchalant treatment of combat psychiatry and of the new nomenclature does not extend to the treatment of psychoanalytic terms. Here the authors have done a painstaking, conscientious, extraordinarily thorough job, almost to the point of a masked nosologic obsession. For example, "stasis, libido" occupies almost a full page of the dictionary, with a bibliographical reference. Most of these words are in the Supplement, where the long definition is the rule rather than the exception. These are not truly definitions, but, rather, discussions of certain terms and phrases used in the literature, usually as they are related to psychoanalysis. There is no objection to this; the result is an excellent reference work on psychoanalysis and its current terminology. The work should have been entitled "Psychoanalytic Dictionary," a much more appropriate index to its contents.

This, then, is a pre-Johnsonian dictionary, defining for us the words of the learned. Gratis, and to its detriment, it includes a moderately accurate dictionary of terms static since 1940, and an obso-

lete, frequently inaccurate reference work on clinical psychiatry.

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#### PSYCHOTIC AND NEUROTIC ILLNESSES IN TWINS.

Medical Research Council, Special Report Series, No. 278. By *Eliot Slater, with the assistance of James Shields*. (London. Her Majesty's Stationery Office, 1953. Price: £1 1s. New York: British Information Services. Price: \$4.75.)

This recent report comprises some 385 pages, the larger part dealing with the schizophrenic group of mental illnesses, though the affective, organic, psychopathic, and neurotic groups are also considered. The study is an outstanding attempt to apply the Twin Method in a genetical analysis of mental illness. The material for statistical analysis consists of 297 twin pairs of which 67 are classified as uniovular. The concordance rate for the schizophrenic group was found to be 76% in uniovular twins and 14% in binovular twins, a considerable difference. This seems, as the authors say, to confirm "previous views on the importance of genetical factors in the psychoses." Being such an extensive study it is one that will receive much praise from many quarters.

However, since the study of human heredity presents such great difficulties, it is not surprising that these reviewers find some of the statements and interpretations controversial. For example, in the introductory discussion of the rationale of the Twin Method, the authors imply that uniovular twins may arise from an early separation at the 2-celled stage, though it is generally accepted that this is unlikely. In the majority of cases the division is certainly delayed until after the chorion has been formed, since the large majority of uniovular twins are monozygotic. Also outmoded is a discussion of the theoretical Third Type Twin, which is supposed to result from the fertilization of a single ovum and its polar body by 2 separate spermatozoa.

More important criticisms can be made of the accuracy of the diagnosis of twinning, which is fundamental to all that follows. The differentiation between uniovular and binovular twins was based, in part, on similarity or history of similarity, and, in part, on anthropometric measurements such as digital patterns, height, hair color, eye color, and skull measurements. Unless the report is examined critically, the reader may fail to notice the large proportion of instances where the diagnosis of zygosity was based only on the first criterion—sometimes on photographs or hearsay, both of which can be misleading. This is illustrated by the fact that of the true total of 61 uniovular twins there were 26 pairs for whom no fingerprints were available; these are all designated "Uniovular." In any case, to base a diagnosis of zygosity on the dermatoglyphics of finger prints alone (disregarding palmar and plantar configurations) is open to question.

In a number of instances, too, the authors seem to have ignored, for reasons the reviewers find unsatisfactory, the verdict of an objective criterion such as fingerprints. The twin pair, No. 4, are described as "never so alike as to be mistaken for each other." The difference in the total digital ridge counts between the co-twins has a value of 0.27, which is greater than the mean value of binovular twins (0.245) and far greater than the mean value of uniovular pairs (0.068), indicating that the pair under discussion are binovular. A similar conclusion is drawn from the value of the discriminant function used by the authors. It seems to the reviewers that a misclassification has therefore been made, these binovular twins being listed as uniovular. They were concordant for schizophrenia.

Issue also may be taken with the uniovular classification of pair No. 107/108, whose "mother denies that they were ever mistaken for each other" and whose father was a binovular twin. The latter fact makes it more probable that these twins were also binovular. By the method employed in this study a double count is given to whorl patterns with the result that this particular pair of twins shows a low value (of 0.02) for the difference between the total ridge counts. By a more standard method of using only one count for a whorl pattern, these reviewers found the difference to be indicative of binovular twinning. The pair has been classified as uniovular and concordant for schizophrenia. Moreover having 2 propositi the pair is counted twice.

The procedure of counting a twin pair twice is followed wherever a pair contains 2 propositi. The authors have applied in effect Weinberg's proband method, to estimate the frequency with which a characteristic exhibits itself in co-twins of the propositi. The resulting rate is described as the "concordance rate," though some other description would seem more satisfactory; for example, Luxenburger's "probability of manifestation." The common term "degree of penetrance" would be suitable if the trait were genetically determined.

The following is a specific example of this procedure. Of the uniovular schizophrenic twins there are 13 discordant pairs and 24 concordant pairs. Since 4 concordant pairs have 2 index cases, a total of 28 concordant pairs were counted. The "concordance rate" is then given as 68.3% ( $28/41 \times 100$ ). This estimate of the frequency with which the characteristic exhibits itself in co-twins was adjusted for differences in the age of onset of the condition to a value of 76%.

It is unfortunate that the procedure is not more fully discussed in the report, since many investigators do not seem aware of its possible application to this type of problem. In most presentations the "crude" concordance rate is given, which is equal to the "probability of manifestation" only if no pair contains more than one index case. This would tend to occur if the probability of detection of an abnormal individual were very small. When, for some pairs, both concordant twins are index cases, the procedure followed here should then be

used, *provided* the abnormal individuals have been detected independently of one another. It would be difficult in many investigations to decide if this were strictly so.

The authors deserve praise for the use they have made of the twin method in this study. They rightly conclude: "Though it has certain limitations, twin research, especially when combined with family investigation and case study, is a valid method for investigating the effects of heredity and environment." The limitations of the method at the present time need, however, to be strongly emphasized. The authors are also to be commended for the way in which they have presented the results of their investigation. It was only because of this detailed and honest presentation that the criticisms given above were possible.

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**DAS AUTOGENE TRAINING (KONZENTRIERTE SELBST-ENTSPANNUNG).** 8th Enlarged Edition. By Prof. J. H. Schultz. (Stuttgart: Georg Thieme Verlag, 1953.)

"Das Autogene Training" means a concentrative self-relaxation. Since 1932 (first edition), Schultz, a well-known German psychiatrist, has been using this type of psychotherapy. In this discipline, the patient has to learn by self-discipline to influence the condition of his body through the mind. The concentrative self-relaxation or meditation of the autogenic training has the purpose to create changes in the body by certain well-described exercises, designed to "make the patient healthier and to diminish unhealthiness." We see that this type of psychotherapy makes use of the theories of self-suggestion, hypnosis, and yoga. In this eighth edition, new philosophic religious studies, especially in the field of Pali Buddhism (Dr. med. Helmut Palmie), are inserted into the text of the book. A great part of the literature about hypnotism, self-suggestion, body image, phantom limb, and body-mind relations in general, is discussed.

It is not easy to summarize this book because of the description of the exercises, the literature, and the protocol about the numerous cases treated by the author and his followers.

In a small booklet published in 1952 (6th edition) the exercises are described in a popular manner.

Ernst Kretschmer, a leading German psychiatrist, called the Autogene Training a great achievement and a new way of psychotherapy which we can call the yoga line. I mention this because, in spite of the tremendous success which the autogenic training has had in Germany in the last 20 years, it is not well known in this country. It might be a good suggestion to have this book translated into English so that some psychotherapists could have their own experience with this form of treatment.

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**L'ASTINENZA SESSUALE. (THE SEXUAL ABSTINENCE.)**  
By Stefano Fajrajzen. Preface by M. Levi Bianchini. (Milano: Fratelli Bocca, 1952.)

In this comparatively small volume (333 pages) the author makes a well-rounded presentation of the problem of "sexual abstinence" from its numerous viewpoints. Each of its 22 chapters is amply supplied with references to various literary sources including those published in English and German, a marked departure from the former Italian "tradition" to limit their bibliographies to Italian and French works.

The complexity of this problem becomes apparent when one follows the author's efforts to arrive at a clear definition of this term. What type of sexual activity do we have in mind when we discuss the abstinence from it? May we include in it libidinal cravings that obtain satisfaction through masturbation, pollutions, spermatorrhea, homosexuality, coitus interruptus, coitus inter femora, and other "deviated" forms?

Must abstinence be voluntary to be considered as such or would it comprise an abstinence resulting from the force of circumstances as found in prisons, seafaring ships, and isolated combat areas? Are there chronological limits of the period of sexual interest in human life, from its first appearance to its final termination, in the light of Freud's teachings? Even the old terms of sexual maturity and climacteric differ in various climates and in various stages of civilization.

The scarcity of males as compared with females in various countries must be considered an important factor in diminished sexual activity. According to the figures for 1945, one of 5 women in great Britain and one of 4 in Ireland will remain out of wedlock. In Australia 68% of males between the ages of 25-29 were unmarried and in Switzerland, 66%.

Is total sexual abstinence injurious to one's physical or mental health? The author quotes numerous investigators. Some deny any such sequelae (Ribbing, Acton, Gowers, Beale, Pagets, Fournier, Eulenburg, Mengazzini, Bumke, Von Monakow, Forel, *et al.*). Others assume that only neurotic individuals may be affected unfavorably by it (Bloch, Lowenfeld, Moll, Kraft-Ebing, *et al.*). Some advise containment only till a certain age (Erb, Beard) while a smaller group of researchers (Gyurkovecky, Jastrowitz, Nystrom, Marcus, Hirschfeld, Stekel, Gattel, and Freud) claim that it may cause anxiety and other emotional disturbances even in the average individual.

The author suggests an outline for the investigation of physical, mental and moral factors in sexual abstinence. He elaborates particularly on the emotional attachment to parents and siblings ("slaves of the family") resulting in attraction to the same sex. Retention of urine or its frequency may also be used for sexual satisfaction. He quotes Freud's unusual theory of "upward displacement," by which inhibited libidinal urges leave the genital area and rise to the head where the vasomotor



symptoms like vertigo, congestions, and pulsations are symbolical surrogates of orgasms.

Reich suggests that even epilepsy is a vicarious orgasm, while to Stekel migraine expresses masked sexual phantasies. Some interpret in the same spirit the manifestations of kleptomania, the zeal of the fanatic and of the reformer, the animosity towards close relatives and the aggressiveness that turns one against himself in the form of guilt feelings and suicide.

In his final chapter the author, who opposes sexual abstinence, suggests various means for combatting it: early marriages, large families, divorce among the incompatibly mated, university professorships on sexology, sex education in schools, and measures against celibacy. The author sees on the horizon the sad possibility of a return to the free primitive sexual relationships as practiced among the savages of Melanesia, in order to reestablish the biological equilibrium of the instinctive forces which civilization has disturbed.

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**NURSE-PATIENT RELATIONSHIPS IN PSYCHIATRY.** By Helena W. Render, R.N. (New York: McGraw-Hill, 1947.)

Embarrassingly enough recovered patients leaving the hospital will often name one of the non-medical personnel, perhaps most often a nurse, as the person most responsible for their recovery. Hence as long as the ward charge holds such a crucial position in the therapeutic team, there will be a great and continuous need for good psychiatric nursing books. Unfortunately much of the literature in this area gives every indication of belonging to the warmed-over-lecture-notes, diploma-mill school of writing.

Not so this book. It reveals an intimate acquaintance with the realities of ward life and is the fruit of long experience. There are defects, to be sure, and minor lapses. Chapter II, for instance, contains a monstrous screen to be used by nurses for the observation of patients (119 items from "active, afraid" to "... vulgar, voluble"), which many readers will feel is unrealistic. In order to become expert in interpreting facial expression, the curious suggestion is made (Chap. IX) that student nurses be taken to the great art galleries to peruse certain canvases of Millet, El Greco, and other masters of whom a list is given. This is a delightful whimsy although the clinical application may be debatable.

The core of psychiatric nursing is defined as "modifying moods and changing attitudes," and the bulk of the book is written around this. Particularly impressive is Chap. III which deals with the patients' fears, especially those of newly admitted patients, and which gives detailed and sound suggestions as to how the nurse can handle this complex problem. Here the author is obviously at home and possibly at her best. There is also excellent coverage of the strategic use of the admission routine, of the business of maintaining flexible and effective nursing attitudes, and the various

techniques of providing security. In the sections dealing with the clinical syndromes, the pages devoted to schizophrenia (back-building, taxpayers, schizophrenia—not the newfangled, psychoanalytic construct) are especially rewarding and give glimpses of a total-push program in actual operation.

This book is the product of long practical experience and at the same time is suffused with a buoyant, therapeutic orientation toward this most difficult branch of nursing. It reflects a writer with a keen sense of dedication.

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**SCIENCE AND MAN'S BEHAVIOR.** By Trigant Burrow, M.D., Ph.D. (New York: Philosophical Library, 1953. Price: \$6.00.)

This is indeed an extraordinary book, extraordinary in the literal sense of the word. It is a posthumous edition, and the editor and faithful disciple, William E. Galt, Ph.D., calls it "the essence of Dr. Burrow's phylobiological researches." Those of us whom Dr. Burrow's earlier books had stimulated, but had left somewhat confused and baffled, have waited eagerly for the final formulations of his concept of "disordered human behavior."

The second part of this volume contains the author's book *The Neurosis of Man*; it has to be read first in order to understand the part which precedes it. This first part reports, in a somewhat dismembered fashion, the reactions of 29 outstanding research scientists from many parts of the world and many and diversified fields of science to whom galley proofs of *The Neurosis of Man*, or parts of it, had been forwarded. It also contains Dr. Burrow's answers, responses, and apologia to their comments. As in his previous books, common concepts and words undergo great changes when used in the author's frame of reference. The neurosis he speaks of has little in common with our use of this concept in everyday psychopathology. Man, i.e., all men, suffer from it, not as individuals but as parts of a superseding unit, "phylum," in which Dr. Burrow is solely interested because he considers it a biological entity with its own physiology and behavioral apparatus. Neurosis is pandemic! Man has lost (or never had) "cotention," i.e., "the neurodynamic relation of organism to environment as it exists natively and undifferentiated within the cerebro-sympathetic system." Instead, man is the victim of "ditution," namely, "the situational reaction in which ulterior advantage supersedes direct interest in an object or condition for itself." This results in "I-persona," i.e., "the restricted part-expression of the personality," ruled by "part-brain: the restricted cerebral system that controls the secondary or partitive pattern of tensions and relates individuals only symbolically, vicariously to one another." This then corresponds, according to Dr. Burrow's nomenclature, to "Parencephalon, third nervous system, third brain."

Dr. Burrow's attempt to create a science of "behavioral medicine" is obviously closely related to



Walter B. Cannon's writings about "automatic regulatory mechanisms of the body physiologic" and their application to the "body politic." (Walter B. Cannon, "The Body Physiologic and the Body Politic," *Scientific Monthly*, Vol. 79, No. 1). Burrow tries with all the spirited energy of the social reformer to develop scientific and especially physiologic proof for his concept that man in his interpersonal relations is on the wrong path, has been on the wrong path ever since history started, and that this is a last call to save man. On the publisher's jacket it says: "Although sweeping in its indictment of current behavioral interpretations, the book is essentially inspiring." But this, being "essentially inspiring," is the tragic blow of death to a man who started out to use neurophysiology as a scientific explanation for his concept of human behavior. Burrow's goal is harmony, permanent harmony, in human relations. He wants to lead man to it, using physiological experimentation as a compass. Man's affect of course interferes—let's do away with it! "The affect is an unwarranted feeling bred of the individual's wishful self-interest or his self-bias" (p. 160). This is the language of the evangelist, or the medieval *Bussprediger*, it has no relation to medical science or physiology.

There is tragedy in this last book of a great and gifted man, a former president of the American Psychoanalytic Association, who describes his increasing withdrawal and isolation from friends and colleagues into the lonesomeness of the Lifwynn Foundation, surrounded only by the devoted family and a few close friends and followers. There are occasional attempts to justify his changed relationship to psychoanalysis and to Freud; the author hardly sees that Freud was essentially a physician, a healer. Dr Burrow finds himself far away from medical psychoanalysis, somewhere between the social reformer, Alfred Adler, and the philosophical mysticism of Carl Jung.

Dr. Burrow had gotten far away from medicine, but the philosophers, sociologists, and anthropologists to whom he submitted this book's galley proofs could not follow his reasoning either. Their baffled and sceptical, polite answers culminate in the late John Dewey's final statement: "In other words, part of my difficulty is, that I have not definitely made out just what your underlying postulates may be."

Thus has ended the life of the brilliant personality of Trigant Burrow in a confused and over-written accusation against the development of modern man, full of neologisms, full of repetitions and contradictions, full of good will and imagination, but tragically lacking scientific organization, discipline, and convincing evidence.

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**CHILD TRAINING AND PERSONALITY: A CROSS CULTURAL STUDY.** By John W. M. Whiting and Irvin L. Child. (New Haven: Yale University Press, 1953. Price: \$5.00.)

Cultural anthropologists are becoming increasingly concerned with personality and its relation-

ship to society. Most of the efforts to deal with this problem on a theoretical level have borrowed rather heavily from the conceptual framework of psychoanalysis. The validity of Freudian metapsychology has been assumed in advance and, consequently, has been employed to interpret the observations obtained in the field rather than having been checked against them. The cultural anthropologist has been disposed to regard the theory of psychoanalysis as clinically verified; unfortunately for this naïve view, objective and reliable clinical evidence is simply nonexistent, if one insists upon the usual standards for scientific proof. The comparative data on child rearing and adult personality provided by the manifold varieties of human society would appear to afford a unique opportunity to test the hypotheses of psychoanalysis.

With circumscriptions of the problem, that is what the volume under review proposes. Having somewhat modified, in the light of learning theory, certain psychoanalytic constructs, the authors have set about testing their validity statistically. An impressive amount of work has gone into this study. At the outset terms are rather clearly defined; methodology and data are fully reported. Commendable efforts have been made to guarantee the objectivity of results.

It is not possible, within the confines of a brief review, to appraise this volume in detail. A serious work, it merits serious consideration, however, certain general questions are raised by this study that are fundamental not only to its evaluation but also to current trends in cultural anthropology.

Drs. Whiting and Child have attempted to assess the degree of correlation that exists between child training practices on the one hand, and the system of beliefs about illness, on the other; the latter taken as an index of personality and considered in relation to the theory of fixation. The raw materials of the study are field reports by others on 70 "primitive" societies, for two thirds of which only one source of information is cited. The reports vary from accounts by early travellers to detailed investigations by contemporary anthropologists, the dates of publication from 1704 to the present. Now cultural data, at best, are subject to biased reporting and evaluation. (Compare Lewis' "Life in a Mexican Village: Tepotzlan Restudied" and the recent report from the Yale group, Gladwin's paper on the Trukese in the *Trans. N.Y. Acad. Sci.* of 1953.) With the variable reliability of the data itself in mind, judgment on a notion such as "severity of socialization" would hardly seem reducible to precise numbers on a scale—a 21-point scale, at that!—whatever the "coefficient of correlation" between three judges' opinions.

There can be no question of the crying need in the disciplines that study human behavior for quantitative rigor. Certainly, the great usefulness of statistical methods elsewhere gives promise that they have much to offer to us. But statistics can be seductive. Once data have been converted to numbers and fed into the statistical hopper to grind out degrees of significance, they are endowed with an "objectivity" that may be more apparent than real. For, it must be clearly understood, statistical methods are no more than useful tools to maximize

the extraction of information that is *already* in the data; they do *not* introduce validity or significance into the raw material. If we have reason to doubt the data—and I am afraid that here we must—the correlation coefficients or “p” values derived after the data have been cloaked in the sanctity of numbers are without relevance to the basic questions we have raised.

There is yet a further trap in the statistical conclusions of this study, one of which the authors seem unaware. Perhaps a hundred correlations are tried for fit in the course of this volume. This being true, the ordinarily significant “p” value of .05 (a 1 in 20 probability of being due to chance) cannot be relied upon, since at least 5 such “significant” correlations will have been *due to chance alone* (out of the hundred tested). Those with a “p” value of .01 give us a bit more confidence, since only one should be “valid” by chance, and only the very few at a confidence level of .001 can be reliably regarded as due to factors other than chance. If we insist on this criterion, as I believe we must, then child training practices with regard to “sex,” “oral,” and “anal” matters would appear to have no bearing on the projective systems of adult personality as measured in this study. “Aggression” and “dependency” training alone appear to matter. This should surely disconcert at least the orthodox analyst—though, as has been indicated, the results can be accepted only with the greatest caution.

There are many other questions this reviewer has. Just how valid is the assumed homogeneity of “primitive” societies, an assumption which is integral to the notion of standardized child training practices and modal personality or belief? Can systems of belief be so simply related to childhood training? What of the role of the priest-physician in perpetuating doctrine as an instrument of power? Correlations, of course, even when positive, tell us nothing about causality; items that vary concurrently may be moving in response to an underlying more fundamental factor. The authors are fully aware of this and correctly point out that a significant correlation can be a guide to further investigation.

In essence, then, this study suffers from the conceptual limitations that plague cultural anthropology in general. It attempts to employ statistical methods, a desideratum in itself, but without full cognizance of the limitations set by unreliability in the data. Nevertheless, it is a welcome pioneer work in a domain that has been marked till now by a wholly qualitative approach and an appeal to “reasonableness” as an argument for validity. Cultural anthropology will have achieved full status as a science when rigorous proof has been substituted for the intellectual tour de force in its struggle to evolve a theory.

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PRINCIPLES OF INTERNAL MEDICINE. Second Edition.  
Edited by T. R. Harrison, R. D. Adams, P. B. Beeson, W. H. Resnik, G. W. Thorn, and M. M. Wintrobe. (New York: Blakiston, 1954. Price: Student 1-Vol. Ed.: \$16.00; Professional 2-Vol. Ed., boxed: \$21.00.)

This publication is put out in 2 editions: a 1-volume edition of 1,703 pages, and an index of 87 pages, which is listed as being for students, and a professional edition of 2 volumes. The editor-in-chief, Dr. T. R. Harrison, is Professor of Medicine at the Medical College of Alabama, but the publication is made up of material from 85 contributors. It is the “aim of this book to present within the confines of a single volume a consideration of the disorders that comprise the province of internal medicine. An attempt has been made to integrate the pertinent content of the preclinical sciences with clinical medicine, and to approach the subject not only from the standpoint of disorders of structure, but also by way of abnormal physiology, chemistry, and disturbed psychology. This method of presentation follows the modern trend in medical education. The book is directed primarily at the student and physician who desire a presentation of the important scientific principles that are necessary for a rational understanding of the development, evolution, and management of internal diseases.”

The editors further point out that “because many constitutional diseases, as well as neurologic disorders, tend to present themselves with manifestations referable to the nervous system, a series of chapters has been written dealing with the more common manifestations of disordered nervous system function.” The table of contents and index make this book a ready reference.

In the section discussing the “Cardinal Manifestations of Disease” there is a detailed discussion of “Pain.” Every physician is faced with the proper evaluation of this major symptom. In “Disorders of Nervous Function” there are extensive discussions on the various symptoms found in such disorders. It is unfortunate that this volume, purporting to be up-to-date, does not follow the pattern of the new classification accepted by the American Psychiatric Association. The book, of course, is presented from the angle of the general practitioner rather than that of the psychiatrist.

In the last section, “The Nervous System,” there is a further discussion of the pathology associated with the nervous system, and with which every psychiatrist should be acquainted.

One of the contributors, Raymond D. Adams, Professor of Neuropathology at Harvard Medical School, well states that “the average physician, upon scrutiny of his own patients, will be forced to admit that between 50 and 75 per cent of them are suffering from pure psychoneuroses or superimposition of a neurotic reaction upon organic disease. This is also true of the patients entering large municipal or private hospitals for diagnostic studies.” Although the author is not a psychiatrist, he appears to be well orientated in modern psycho-

pathology but somewhat biased in his opinion, for he states that "it would be erroneous to conclude that the psychic factors are causative."

In spite of the fact that the psychiatrist will find much with which to disagree in this volume, the editors have taken a step in the right direction. This book would definitely be a contribution to the library of every psychiatrist.

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**PSYCHOANALYSIS AND PERSONALITY.** By *Joseph Nuttin.* (New York: Sheed and Ward, 1953. Price: \$4.00.)

The dimensions of sin, like the dimensions of the universe, have changed with the advance of knowledge. At first the expanding cosmology of the astronomers aroused intense anxiety in the souls of the dogmatically faithful, and it became in due course a subject for theological disputation or inquisitions concerning heresy. In the end, superstition was abandoned, and, as history has demonstrated, the official basis for morality was not undermined. In the realm of man's knowledge concerning himself, Freud's psychoanalytic discoveries have proven equally expansive, equally revolutionary, and equally disturbing.

Psychoanalysis is fraught with far-reaching implications for any closely knit and systematized religion, implications involving both practice and doctrine. How, for example, is the degree of culpability altered by the awareness of unconscious motivation? Where in the hierarchy of sinfulness does the genuinely unconscious sinner fit? What is the value of the self-imposed penitential act (e.g., as in compulsion neurosis) when the act is secretly a hostile caricature of officially sanctioned penance? These are but a few of the many problems that psychoanalytic insight may raise for the priest, the moralist, the religious psychotherapist, social worker, etc.

As in the case of its counterpart in cosmology, the response to this historical development in psychology has been varied. The discoveries of psychoanalysis have been disregarded, attacked as a tissue of lies, labeled completely unscientific, or an evil ex-crescence, symbolic of the moral decay of our age. Fortunately such is not the approach of Joseph Nuttin, Catholic professor of psychology at the University of Louvain, in this book. Nuttin acknowledges the reality of the impact of psychoanalysis, emphasizing how its concepts of motivation, dynamic conflict, and personality have transformed modern psychology.

This book consists of two parts. In the first, the author attempts to describe and evaluate psychoanalytic theory, its philosophical assumptions, and therapeutic approach. In the second, he advances a "new" dynamic theory of normal personality, not based on the findings of psychopathology exclusively and taking into account man's spiritual needs. (There is in addition a 16-page appendix outlining Adler's individual psychology.)

In both portions of the book, the author achieves only partial success. At best his elaboration of psy-

choanalytic theory is distant and non-comprehensive, primarily because he concentrates on the discoveries stemming from the earliest period of psychoanalytic history, namely the psychology of hysteria, repression, and infantile sexuality. The complexity of the operations of the ego, its regard for reality and for morality, are missed. Repeatedly the author refers to Freud's method of reducing psychic phenomena to the manifestations of one force, the libido, which is obviously incorrect. The author furthermore confuses the working hypotheses of psychoanalysis, the theory of drives (to which most Freudian analysts subscribe) with Freud's broad philosophical speculations concerning the life and death instincts (with which many Freudian analysts disagree). From the very few clinical examples which the author cites concerning analytic experiences it becomes clear that his grasp of analytic principles is more scholarly than real. (In analyzing the case of a young girl, Alma, in conflict between marriage and chastity, the author describes an apparently unmotivated outburst of tears which the patient experienced while lying in bed with a girl friend who "had also become the object of Alma's need for erotic affection." No mention is made of this homosexual temptation in evaluating the material.)

The author's own theory of the normal personality is eclectic in derivation. As a serious minded scientific worker, Nuttin has attempted to integrate some of the contributions of Freud, Adler, K. L. Swin, Allport, and many others with the philosophical and moral principles fundamental to his faith. Basic is the concept of each man as an individual core of life "formed and fed by contact with the non-ego" and manifesting a need for others. These forms of exchange are experienced at various levels, biological, psycho-physiological, psycho-social, and spiritual. The "law of effect" dictates "that normal forms of behavior, and the dynamic forces which lie behind them, tend to develop according to the success achieved . . . forms of behaviour which lead to a satisfactory result are maintained by the organism," unsatisfactory forms are increasingly eliminated. In this fashion the drive for self-preservation and self-development fuse with the need to be linked to other beings. "This complex double tendency—towards self development and towards contact with others—can therefore be considered the dynamic expression, on the psychic level, of what man is."

This book may be a modest but significant beginning of an important development, the integration of the technical contribution which depth psychology has to offer with the problems of human sin and faith in the context of a religious frame of reference.

JACOB A. ARLOW, M.D.,  
New York City.

**INTRODUCTION TO PSYCHIATRIC OCCUPATIONAL THERAPY.** By *Gail S. Fidler, O. T. R. and Jay W. Fidler, M.D.* (New York: Macmillan, 1954. Price: \$4.00.)

The authors of this 200-page book are married and their training seems to have fitted them well for

the task they set themselves in elaborating their subject. Mrs. Fidler is a graduate of the Philadelphia School of Occupational Therapy. Both she and Dr. Fidler studied at the William Alanson White Institute of Psychiatry, and she has had a number of years experience in at least 4 hospitals, has served as Chief O. T. in 2 of them.

In the Introduction by Elizabeth P. Ridgway, O.T.R., it is stated: "The psychiatric frame of reference of the authors is that of Harry Stack Sullivan and the William Alanson White Foundation," (and this is quite evident in the text that follows). Miss Ridgway then goes on to say: "In a sense this is quite unimportant because the material used is not controversial if one accepts the idea of human beings developing dynamically."

It may be said that the authors have been successful in developing their thesis, and that students of occupational therapy will gain in understanding present-day trends in treatment of psychotic patients. However, it would seem that, in order to insure better cooperation between the prescribing psychiatrist and the therapist, the former should study this book more thoroughly than the pupil in therapy.

The subject of the therapist's records is well covered and should be of material assistance to the psychiatrist in understanding the psychosis of the patient by the information gained from them. The atmosphere of an occupational therapy clinic is usually less formal than that of the ward or psychiatrist's office, and patients are usually less guarded in expressing themselves freely or naturally.

Stereotyped charts are condemned, yet the only example of a chart given is a "progress report" on which degrees of "Hostility, Sociable," etc., are scored each month by the numerals, 0, 1, 2, 3, zero indicating absence of a quality. Presumably such a chart forms a convenient way of rapidly estimating a patient's progress.

The directions for prescription writing seen admirable, and examples emphasize their clarity. If the attending psychiatrist can be persuaded to follow this pattern much better rapport between psychiatrist and therapist will result. Unfortunately, too many psychiatrists write inadequate prescriptions for Occupational Therapy and some (we have heard) refuse to write any, thus placing the therapist in an uncomfortable position and himself in an uncooperative one, also laying himself open to the suspicion that he does not know how to write one. From the fullness of the examples given it may be surmised that the busy psychiatrist will object to such forms as being too time-consuming, losing sight of the fact that such careful summaries prove he has made a careful study of his patient and will help the therapist in her treatment of the patient.

While a few other criticisms might be made, such as, narrow page margins on their binding sides, which force the reader to do a part of his reading on a curved surface. This is probably the fault of the printer and not of the authors. Also the list of

references is heavily loaded with "dynamic" psychiatry, with practically no mention of less radical views of successful therapists and psychiatrists, and little attention is given to other modalities than arts and crafts.

But despite such minor criticism, it may be reiterated that the Fidlers have produced a book of value to both therapists and psychiatrists which should be read by all specializing in the care of mental patients.

W. R. D.

#### CARICATURES OF 88 PIONEERS IN PSYCHOANALYSIS.

Drawn from life by *Olga Székely-Kovács* and *Robert Bereny*. (New York: Basic Books, 1954. Price: \$4.50.)

An entertaining book, and not without historic interest too. The historic interest leads back to 1908 in which year the first congress of psychoanalysts had been held in Salzburg, Austria. Forty-two persons took part in that meeting. In 1924, the psychoanalysts convened again in Salzburg; it was the Eighth International Congress this time, and under the presidency of Dr. Ernest Jones. Of those in attendance, 9 had been present at the first meeting. President Jones reported that the total membership of the International Association was 263. The United States furnished the largest contingent—57 members. The president further noted that since the first Congress, "Some have seceded from the movement [Adler, Stekel, Jung, Ricklin, Maeder], several have to some extent lost interest." Freud, recuperating from an operation, was unable to be present.

During this 1924 meeting the two artists were busily engaged in sketching the profiles, three-quarter, or full faces of the delegates, as might be most impressive. The publisher's note does not say whether the artists were invited or volunteered their services. The male artist did 55 of the drawings, the woman 33. It may be worthy of mention that of the few least unflattering caricatures a majority were done by the male artist.

Women were well represented at this Congress; they were the subjects of 28 of the 88 caricatures. At least 6 of the men and 12 of the women were not physicians. The number of nonmedical participants may have been greater as the title "Dr." prefixed to most of the names is equivocal and may stand for "Ph.D.," as in the case of Dr. Rank, a later secessionist.

The original drawings were presented to Freud and a strictly limited, privately printed edition was issued for the participants. The present reissue constitutes therefore the first edition of these choice works of art available to the public.

Patients are sometimes nervous or timorous in consulting a psychiatrist. To alleviate such discomfort might this book perhaps find its place on the table in the psychiatrist's waiting room?

C. B. F.



## IN MEMORIAM

RILEY H. GUTHRIE, M. D.  
1895-1954

On October 23, 1954, Dr. Riley Henry Guthrie, a U. S. Public Health Service psychiatrist and a Life Fellow of The American Psychiatric Association, died at his home in Bethesda, Maryland. His passing, at the age of 59, ended a career in psychiatry—as program specialist, teacher, and hospital administrator—which spanned nearly 3 decades.

For the past 7 years, Dr. Guthrie was special consultant for mental hospitals for the National Institute of Mental Health, Public Health Service. In this capacity he worked closely with hospital administrators and program executives throughout the states. His reputation as an authority on inpatient facilities for the mentally ill, and the growing public and professional interest in mental hospitals, led state and territorial mental hospital authorities to request his consultative services with increasing frequency. Because of his knowledge and experience, Dr. Guthrie was instrumental in strengthening current hospital practices and pointing the way to achievement of higher standards of services in the state mental hospital systems.

It is appropriate that Dr. Guthrie, who was particularly concerned with improving commitment laws, should see the first steps toward more scientific commitment proceedings taken during his own lifetime. This advance came when the National Advisory Mental Health Council, in response to urgent requests from medico-legal authorities, recommended formulation of a model law on hospitalization of the mentally ill. Experts from the medical, legal, and judicial professions were invited to contribute their advice in drafting a uniform code which would discard the penological concept of the commitment procedure and substitute one of treatment which would be effective, sensible and humane. Dr. Guthrie, because of his extensive knowledge of the problem, was chosen to represent the National Institute of Mental Health on the drafting committee. The finished work, entitled *A Draft Act Governing the Hospitalization of the Mentally Ill*, drew hundreds of requests from Governors, state

legislative bodies, judges and court officials, agency heads and program officials, hospital staffs, county and municipal authorities, voluntary groups, and others interested in achieving a more enlightened attitude toward hospitalization of the mentally ill.

Dr. Guthrie was born in Smithville, Arkansas, and was educated at the University of Arkansas and the University of Tennessee, receiving his M. D. at the latter school in 1921. On completion of his internship at Arkansas State Hospital for Nervous Diseases, he began his professional career at Massillon (Ohio) State Hospital in 1927. The following year he took a post at Boston Psychopathic Hospital. For 11 years he remained in Massachusetts—successively at Monson State Hospital, as Assistant to the Commissioner, State Department of Mental Diseases, and again at Boston Psychopathic Hospital as Chief Executive Officer.

Before joining the Public Health Service, he was on the staff of St. Elizabeths Hospital in Washington, D. C., for 6 years. He served as superintendent of Norwich (Connecticut) State Hospital from 1945 to 1948, when he left to accept the post which he held until his death.

In the field of teaching, he has been clinical professor of psychiatry at Georgetown University Medical School, assistant clinical professor of psychiatry at Yale Medical School, and professor of psychiatry at the University of Connecticut.

In addition to his membership in The American Psychiatric Association, Dr. Guthrie was a Fellow of the American Medical Association, the New England Society of Psychiatry, the Massachusetts Psychiatric Society, and the American Psychopathological Association. During World War II, he was a member of the Regional Medical Advisory Board for Selective Service in Connecticut. Dr. Guthrie was also author of a number of scientific and professional articles on psychiatry and the planning and organization of mental hospital facilities.

He is survived by his widow, Mildred W. Guthrie, and two sisters.

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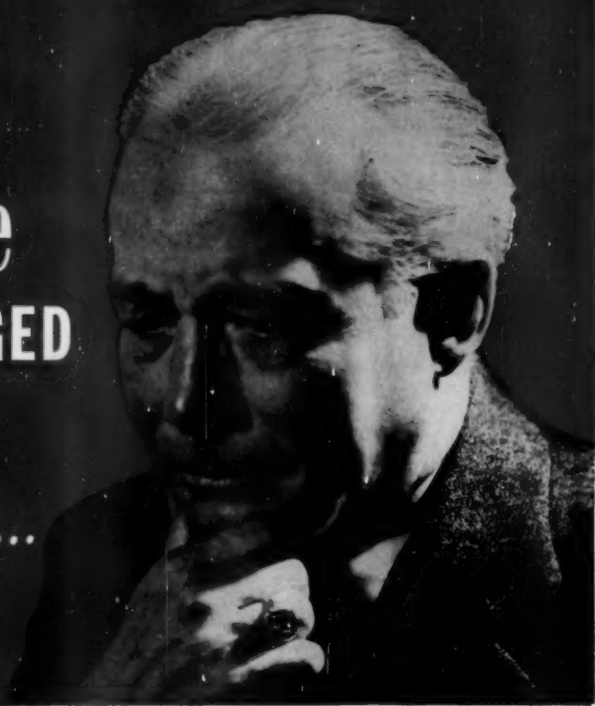


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1. Doyle, P.J., and Livingston, S.: J. Pediat. 43:413 (Oct.) 1953.
2. Forster, F.M.: M. Ann. District of Columbia 23:137 (Mar.) 1954.
3. Lambros, V.S.: Personal Communication.

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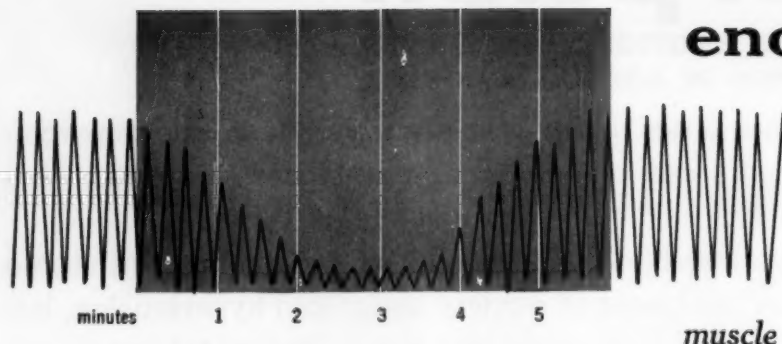
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Holmberg, G. and Thesleff, S.: Am. J. Psychiat. 108:842, 1952.

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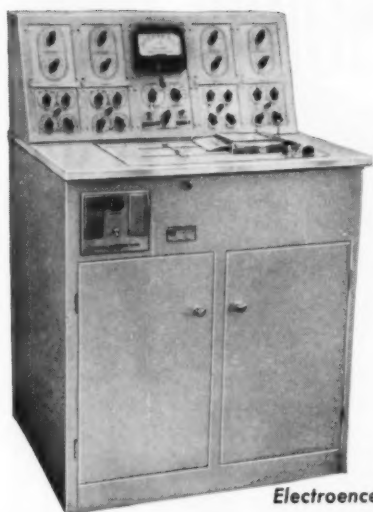


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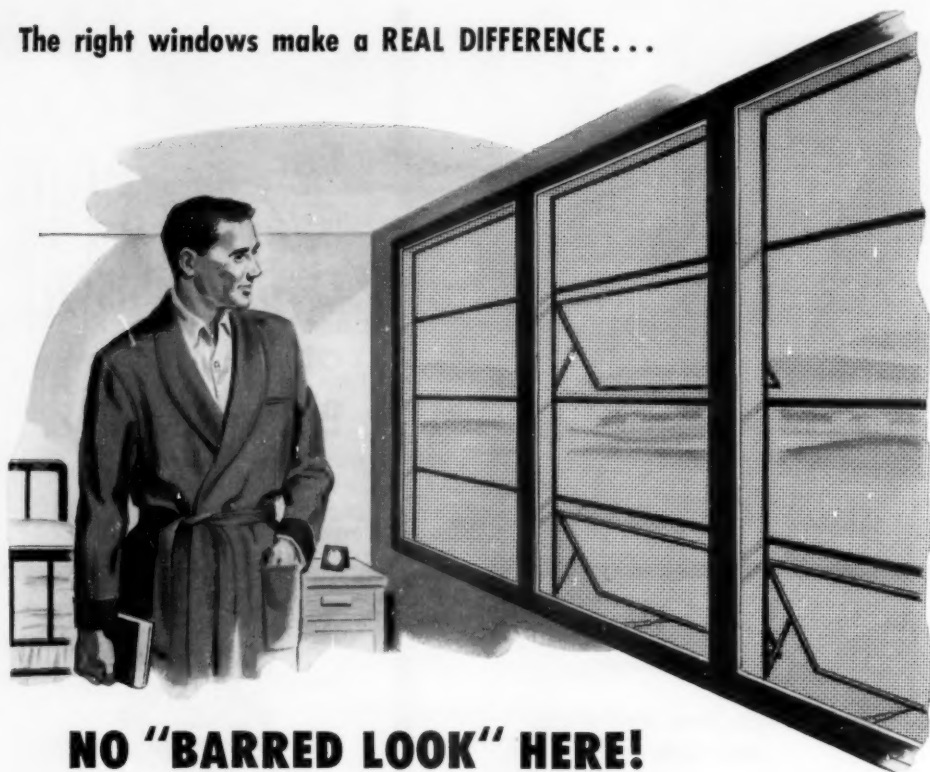
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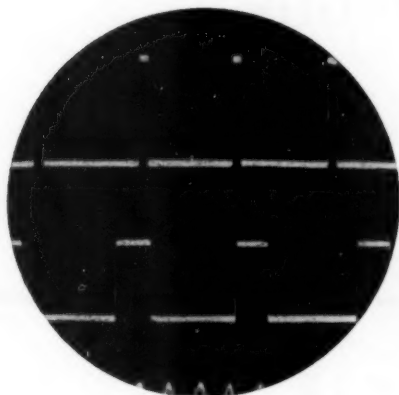
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1. Gerhard Hirschfeld and Joseph Bell *Diseases of the Nervous System* 12: 3-7, September, 1951.
2. Gerhard Hirschfeld *Journal of Nervous and Mental Diseases* 117: 323-328, April, 1953.
3. W. T. Liberson *Psychiatric Treatment* Vol. 31 of Proc. A.R.N.M.D. Williams and Wilkins, Baltimore, 1953.

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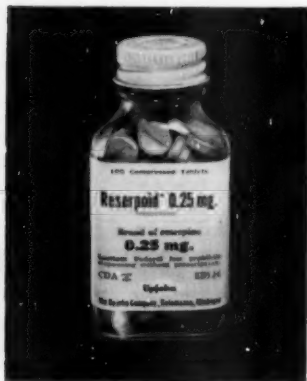
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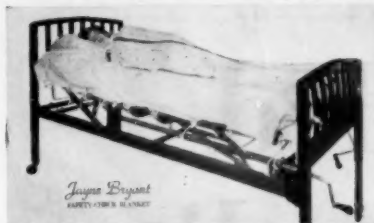
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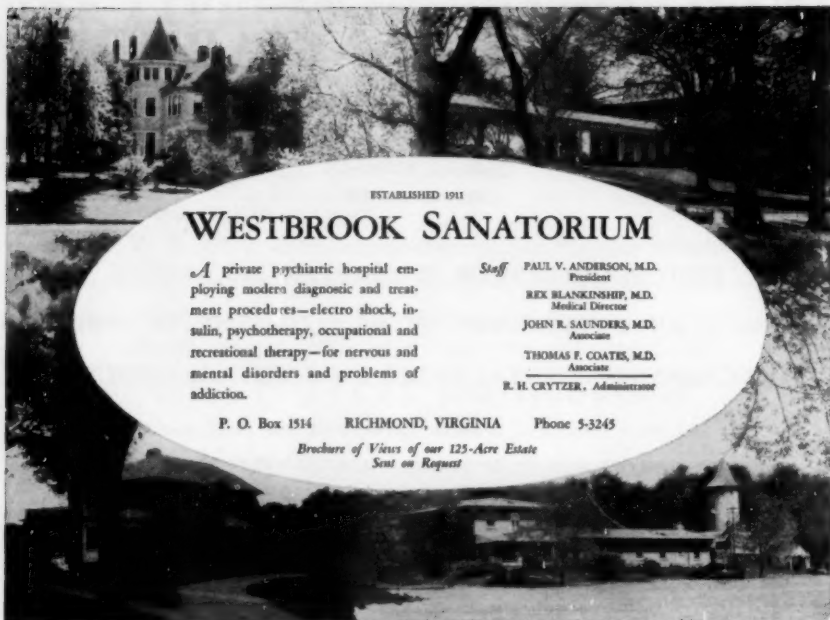
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